Essays on the Economics of People and Places

by

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For Laura

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ABSTRACT

This dissertation contains three essays on the economics of people and places. The essays share a common goal of understanding the long-run consequences of economic and social processes on people, places, and the economy. The essays also share a common approach of combining newly available administrative data with transparent empirical methodologies.

The first chapter paper examines the long-run effects of the 1980-1982 recession on educational attainment and income. Using confidential Census data linked to county of birth, I relate cross-county variation in the severity of the recession to differences in long-run outcomes between individuals who were younger versus older when the recession began. Individuals who were born in counties with a more severe recession and were children or adolescents during the recession are less likely to obtain a college degree and, as adults, earn less income. My estimates, combined with the large number of potentially affected individuals, suggest that the 1980-1982 recession could depress economic output today. Every U.S. recession since 1973 resembles the 1980-1982 recession in persistently decreasing earnings per capita in negatively affected counties, which suggests that other recessions might also have significant long-run effects.

The second chapter, with Evan Taylor, examines the role of social interactions in location decisions. We study over one million long-run location decisions made during two landmark migration episodes by African Americans born in the U.S. South and whites born in the Great Plains. We develop a new method to estimate the strength of social interactions for each receiving and sending location. Social interactions strongly influenced the location decisions of black migrants, but were less important for white migrants. Social interactions were particularly important in providing African American migrants with information about attractive employment opportunities and played a larger role in less costly moves.

The third chapter, also with Evan Taylor, estimates the effect of social connectedness on crime across U.S. cities from 1960-2009. We use a new source of variation in social connectedness stemming from social interactions in the migration of millions of African Americans out of the South. Cities with higher social connectedness had considerably fewer murders, rapes, robberies, assaults, burglaries, and larcenies, with a one standard deviation increase in social connectedness reducing the murder rate by 14 percent. As predicted by a simple economic model, effects on city-level crime rates are stronger in cities with a higher African American population share.

CHAPTER I

The Long-Run Effects of Recessions on Education and Income

1.1 Introduction

Do recessions have long-run effects on education and income? Understanding the determinants of human capital attainment and labor market productivity is a high priority for researchers and policy makers, but there is little direct evidence on this question (Almond and Currie, 2011; Heckman and Mosso, 2014). Long-run effects of recessions on education and income could have substantial welfare consequences, given recessions' frequency and the possibility that each recession affects millions of people. Furthermore, these effects could inform issues of long-standing interest and debate, including the welfare costs of recessions and the relationship between recessions and subsequent economic growth (e.g., Schumpeter, 1939, 1942; Lucas, 1987; Caballero and Hammour, 1994; Davis, Haltiwanger and Schuh, 1996; Barlevy, 2002; Lucas, 2003; Yellen and Akerlof, 2006; Foster, Grim and Haltiwanger, 2016).

This paper provides new evidence on the long-run effects of recessions on education and income. I focus on the 1980-1982 double-dip recession, which followed large increases in the price of oil and interest rates.[1] The recession was concentrated in certain industries, like durable goods manufacturing and wood products, and counties with pre-existing specialization in these industries experienced a more severe recession. This setting is valuable because the severity of the reces-

[1]The NBER recession dates are January to July 1980 and July 1981 to November 1982. I treat these as a single episode.

sion generated substantial variation in local economic activity, and its timing permits the study of pre-recession economic conditions and individuals' long-run outcomes. Notably, I show that the recession led to a persistent relative decrease in local earnings per capita, employment-population ratios, and median family income in negatively affected counties.

I estimate the long-run effects of the 1980-1982 recession on individuals who were children, adolescents, and young adults when the recession began using a generalized difference-in-differences framework that leverages newly available confidential data. I link the 2000 Census and 2001-2013 American Community Surveys to the Social Security Administration NUMIDENT file, which provides adult outcomes and county of birth for 23 million individuals born from 1950-1979. The first difference of my empirical strategy compares the long-run outcomes of individuals born in counties with a more versus less severe recession. I isolate the effect of local labor demand shifts that emerged during the recession by instrumenting for the 1978-1982 change in log real earnings per capita with the log employment change predicted by the interaction of a county's pre-existing industrial structure and aggregate employment changes.[2] The second difference compares outcomes of individuals who were younger versus older when the recession began. Individuals who were 29 years old in 1979 largely completed their schooling before the recession, so they form a valuable comparison group for estimating the effect of the recession on education. However, the recession might have led to a lasting income decrease for older individuals in my comparison group, which means that the estimated effect of the recession on younger individuals' income could be biased upwards.

Economic theory does not provide a sharp prediction about whether the persistent decline in local economic activity that emerges during a recession will increase or decrease educational attainment and income. Parental investments in childhood human capital could rise or fall, as both the opportunity cost of spending time with children and income decline, and community investments could fall, due to a decline in school or neighborhood quality. In the presence of credit constraints, a recession could limit individuals' ability to pay for college. At the same time, a

[2]Following Freeman (1980) and Bartik (1991), many papers use this instrumental variable strategy to study local labor markets (e.g., Blanchard and Katz, 1992; Bound and Holzer, 2000; Notowidigdo, 2013; Diamond, 2016).

recession could increase educational attainment by lowering the opportunity cost of schooling.

I find that the 1980-1982 recession led to sizable long-run reductions in education and income. Individuals who were born in severe recession counties and were children or adolescents during the recession are less likely to obtain a college degree, especially from a four-year college. Consistent with some substitution from four- to two-year colleges, they are more likely to have a two-year degree. I find little evidence of an effect on college attendance or high school graduation. The negative effects on four-year college graduation are most severe and essentially constant for individuals age 0-13 in 1979. This age profile suggests that the underlying mechanisms are a decline in childhood human capital or a cumulative decline in parental resources to pay for college. Because I estimate small and insignificant effects on college graduation for individuals age 14-19 in 1979, short-term credit constraints in paying for college appear to be less important. As adults, individuals born in severe recession counties earn less income and have higher poverty rates. For individuals age 0-10 in 1979, my estimates imply that a 10 percent decrease in real earnings per capita during the recession, which is slightly smaller than one standard deviation, leads to a 3.0 percentage point (9.4 percent) decrease in four-year degree attainment, a $1,314 (3.2 percent) decrease in earned income, and a 1.7 percentage point (13.9 percent) increase in the probability of living in poverty.

Several pieces of evidence support the validity of my empirical strategy. First, I show that economic activity evolved similarly from 1969-1979 in counties with a more versus less severe recession. Second, I find no evidence of a relationship between the severity of the recession and the evolution of maternal education, infant birth weight, or infant mortality for births before 1980. Because I study individuals born before the recession, my results are unlikely to be influenced by selective pre-recession migration or fertility. Finally, I conduct placebo tests by estimating the effect of the recession on education for individuals age 23-28 in 1979, using 29 year olds as a comparison group. Reassuringly, I find no evidence of an effect for 23-28 year olds, who mostly completed their schooling before the recession.

My estimates, combined with the large number of potentially affected individuals, suggest that the 1980-1982 recession could depress aggregate economic output today. I construct back of the envelope calculations that scale my estimates by the 105 million individuals born from 1951-1979. Depending on the assumed evolution of earnings per capita in the absence of the recession, these calculations suggest that the 1980-1982 recession led to 0.9-2.1 million fewer four-year college graduates, \$64-\$145 billion less earned income per year (as of 2000-2013), and 0.5-1.3 million more adults living in poverty each year. These numbers amount to 1.3-3.0 percent of the stock of college-educated adults in 2015, 0.4-0.8 percent of 2015 GDP, and 1.3-2.9 percent of the number of individuals in poverty in 2015. As these simple calculations depend on extrapolating difference-in-differences estimates, they could understate or overstate the true aggregate effects. Nonetheless, my results provide evidence of a new channel through which recessions could affect welfare and economic growth.

This study contributes most directly to the literature examining whether economic conditions affect individuals' long-run outcomes. Using country- and state-level variation, Cutler, Huang and Lleras-Muney (2016) and Rao (2016) find a positive relationship between economic activity in childhood and later-life education and income.[3] These papers estimate the effect of a temporary positive or negative deviation of economic activity from trend, while I focus on variation arising from a recession. This distinction matters because, as I show, recessions in the U.S. generate persistent relative decreases in local economic activity. Other work, based mainly on downturns in the 19th and early 20th centuries, yields mixed evidence on whether recessions affect late-life health (van den Berg, Lindeboom and Portrait, 2006; Cutler, Miller and Norton, 2007; Banerjee et al., 2010; Cutler, Huang and Lleras-Muney, 2016). While recessions reduce college graduates' earnings for several years (Kahn, 2010; Oreopoulos, von Wachter and Heisz, 2012), the broader long-run effects of recessions are uncertain.

This study complements the mixed evidence on whether parental job loss affects children's

[3]Using several European surveys, Cutler, Huang and Lleras-Muney (2016) find that childhood exposure to a positive nationwide GDP fluctuation is associated with more income and education in adulthood. As they acknowledge, a limitation of their empirical strategy is that they cannot control flexibly for non-economic determinants of long-run outcomes that vary at the country-by-birth cohort level. Rao (2016) uses American Community Survey data to relate long-run outcomes to the unemployment rate in individuals' state of birth during childhood. The estimates are sensitive to controls for time-varying region- and state-specific determinants of long-run outcomes.

long-run education and income (Page, Stevens and Lindo, 2007; Bratberg, Nilsen and Vaage, 2008; Oreopoulos, Page and Stevens, 2008; Coelli, 2011; Hilger, 2016). These papers do not directly assess the long-run effects of recessions, which might operate through additional channels besides parental job loss, such as schools, neighborhoods, and peers. This study also complements recent work by Chetty and Hendren (2016*a*,*b*) documenting the cross-sectional characteristics of places that lead to improved economic outcomes for children. Unlike Chetty and Hendren (2016*b*), my results indicate an important role for local economic activity in children's long-run outcomes. Finally, this paper builds on a vast literature examining how economic conditions affect individuals in other ways.[4]

This paper shows that the 1980-1982 recession persistently decreased earnings per capita in negatively affected counties, and individuals born in these counties who were children or adolescents during the recession have less education and income as adults. I also show that every U.S. recession since 1973 resembles the 1980-1982 recession in persistently decreasing earnings per capita in negatively affected counties, which suggests that other recessions might have significant long-run effects. Similar long-run effects could arise from other shocks leading to a persistent decline in local economic activity, such as Chinese import competition (Autor, Dorn and Hanson, 2013) and NAFTA (McLaren and Hakobyan, 2016).

1.2 Background: The 1980-1982 Recession

This paper uses within-state variation in the severity of the 1980-1982 recession driven by counties' pre-existing industrial structure. Certain industries, like durable goods manufacturing and wood products, experienced large employment losses in response to the rapid increase in inter-

[4]Previous studies examine the contemporaneous effects of recessions on health and children (Ruhm, 2000; Chay and Greenstone, 2003; Dehejia and Lleras-Muney, 2004; Ananat et al., 2013; Currie, Duque and Garfinkel, 2015; Lindo, 2015; Ruhm, 2015; Stevens et al., 2015; Golberstein, Gonzales and Meara, 2016; Page, Schaller and Simon, 2016), the effects of job displacement on adults (Jacobson, LaLonde and Sullivan, 1993; Stephens, 2001, 2002; Charles and Stephens, 2004; Sullivan and von Wachter, 2009; Davis and von Wachter, 2011; Schaller and Stevens, 2015), and the short-run effects of parental job displacement on children (Lindo, 2011; Rege, Telle and Votruba, 2011; Stevens and Schaller, 2011; Schaller and Zerpa, 2015). More broadly, a large literature studies the relationship between parental income and children's outcomes; Solon (1999) and Black and Devereux (2011) provide recent reviews, and Løken, Mogstad and Wiswall (2012) use the oil boom in Norway to study this relationship.

est rates, the appreciation of the U.S. dollar and associated import competition, high oil prices, and the decline in aggregate demand. Historical accounts suggest that the recession led to a persistent decrease in local economic activity in some counties. For example, the recession pushed employers to shut down tire factories with dated production technology across the U.S. (Behr, 1980), and many lumber mills in the Northwest closed permanently, due in part to their near-obsolescence after 30 years of use (Wells, 2006). Consistent with these accounts, I show that the 1980-1982 recession generated a sharp decrease in local economic activity in negatively affected counties, and that this decrease was persistent on average.

1.2.1 Evidence of a Sharp, Persistent Decrease in Local Economic Activity

My primary measure of local economic activity is earnings per capita for a county's residents. This is available from the Bureau of Economic Analysis (BEA) Regional Economic Accounts starting in 1969. The earnings variable, which primarily comes from administrative unemployment insurance and tax data, is comprehensive: it includes income from the labor market and asset ownership, but does not include government transfers. The denominator of earnings per capita comes from Census annual population estimates. Throughout, I use the CPI-U to express all monetary variables in 2014 dollars. I also use BEA data on county-level employment and Census County Business Patterns (CBP) data on county-by-industry-level employment.[5] BEA and CBP employment data do not distinguish between full- and part-time jobs and, unlike the earnings data, are reported by county of work.

For each county, I measure the severity of the recession as the 1978-1982 decrease in log real earnings per capita. This variable captures several ways a recession might affect a county's residents, such as extensive margin employment changes, replacement of full-time with part-time jobs, replacement of high-wage with low-wage jobs, and decreasing wages or hours within a job. The NBER dates the start of the first recession as January 1980, but I use 1978 as the pre-recession

[5] CBP data frequently suppress employment for county-by-industry cells to protect respondent confidentiality, but never suppress the number of establishments within establishment size categories. As in Holmes and Stevens (2002), I impute CBP employment using the number of establishments and nationwide information on employment by establishment size. See Appendix A.1 for details.

year because some economic indicators, including earnings per capita, began to decline in 1979.

Figure 1.1 shows that the 1980-1982 recession generated a sharp, persistent relative decrease in earnings per capita in negatively affected counties. The figure plots population-weighted mean real earnings per capita from 1969-2002 for counties with a recession more and less severe than the nationwide (1978 population-weighted) median.[6] To focus on the evolution of earnings per capita over time, I shift the less severe recession line down by $2,110 so that the two lines are equal in 1978. Mean earnings per capita evolves identically in more versus less severe recession counties from 1969-1978, then diverges sharply with the onset of the recession. From 1978-1982, mean real earnings per capita falls by $2,708 (10.4 percent) in more severe recession counties, while increasing by $44 (0.2 percent) in less severe recession counties. After 1982, mean earnings per capita evolves similarly in both sets of counties, including during later recessions, leaving severe recession counties with a persistent relative decline. The employment-population ratio displays a similar pattern (Appendix Figure A.2).

The persistent relative decrease in earnings per capita in Figure 1.1 might seem surprising, given the conventional wisdom that local wages and employment rates steadily converge after negative labor demand shocks (Blanchard and Katz, 1992). However, several studies find lasting wage and employment rate reductions (Bartik, 1991, 1993; Bound and Holzer, 2000; Greenstone and Looney, 2010; Autor, Dorn and Hanson, 2013; Dix-Carneiro and Kovak, 2016; Yagan, 2016), and economic forces can rationalize this finding.[7] For example, in areas experiencing a decline in comparative advantage, a recession could trigger a lasting reduction in economic activity by inducing employers to pay fixed adjustment costs and shut down or move to other areas.[8] Another

[6] I limit the figure to 2002 to focus on years that are most relevant for long-run effects on educational attainment, as the youngest cohort in my sample is 23 years old in 2002. Appendix Figure A.1 contains results for 1969-2013.

[7] My results on the persistence of the 1980-1982 recession for counties agree closely with Greenstone and Looney (2010), but differ from the conclusion of Feyrer, Sacerdote and Stern (2007), who find rapid recovery of unemployment rates following steel and auto job losses. I discuss the relationship between my work and these papers in detail in Appendix A.2.

[8] Foote (1998) discusses this point in the context of a (S, s) adjustment model with trend growth. Recent findings similar in spirit to Figure 1.1 are that almost all of the decline in routine employment since the early 1990's and almost all of the decline in hires and separations since 2000 have been concentrated in recessions (Jaimovich and Siu, 2015; Hyatt and Spletzer, 2013). This explanation appears in historical accounts of the tire and lumber industries (Behr, 1980; Wells, 2006).

possible explanation is a persistent shift in labor demand at the industry level; the U.S. trade deficit widened after 1982, following a period of high interest rates, large budget deficits, and a strong dollar.[9] The tendency of high earnings individuals to out-migrate more following a decrease in local labor demand could also contribute to a lasting decrease in earnings per capita (Topel, 1986; Bound and Holzer, 2000; Notowidigdo, 2013).

Figure 1.2 displays the considerable cross-county variation in the severity of the 1980-1982 recession. Categories on the map correspond to deciles, with darker shades of red indicating a more severe recession. Twenty percent of counties experienced a decline in earnings per capita of 16.5 percent or more, while twenty percent grew.[10] Clear regional patterns stand out: oil-exporting states, like Kansas, Oklahoma, and Texas, benefited from high oil prices, and states specializing in durable goods manufacturing, like Indiana, Michigan, and Ohio, saw particularly large earnings decreases. New England, with more high tech and defense-related manufacturing, fared relatively well, while the Pacific Northwest, which specialized in logging, fared poorly. Parts of the agricultural upper Midwest also fared poorly, in conjunction with the "farm crisis" (Barnett, 2000). Although the regional patterns are striking, 92 percent of the variation in the severity of the recession is within-region and 63 percent is within-state.

1.2.2 Pre-Existing Industrial Specialization and Recession Severity

Earnings per capita in a county might have decreased from 1978-1982 because of a decrease in labor demand or an unrelated decrease in the share of high income residents (i.e., a labor supply shock). As discussed in Section 1.3, a labor demand shock might affect children's long-run

[9] The auto industry illustrates both explanations. By the early 1970's, foreign automakers - primarily from Japan and specializing in small, fuel-efficient cars - established a stable presence in the U.S. market. Imports' market share rose from 18 percent in 1978 to 27 percent in 1980 along with the price of gasoline. Although the price of gasoline returned to its pre-recession level, the market share of domestic automakers did not. Foreign automakers established U.S. production facilities, starting with Volkswagen in 1978 and followed by five Japanese automakers from 1982-1989 (Klier, 2009). Foreign automakers did not build their facilities in traditional car-making locations: Honda's facility was in Marysville, Ohio, and Toyota's in Georgetown, Kentucky. Alder, Lagakos and Ohanian (2017) also note that Rust Belt firms faced more competitive pressures in the 1980's due to entry from domestic and foreign firms.

[10] The unweighted average decrease in log real earnings per capita is 7.4 percent, and the standard deviation is 12.0 percent. Using 1978 population weights, the average decrease is 5.8 percent, and the standard deviation is 7.6 percent. The histogram of log earnings per capita changes closely approximates a normal distribution (Appendix Figure A.3).

outcomes via parents' budget constraint, community investments, and the opportunity cost of education. An unrelated labor supply shock might affect some of these channels, but the resulting effects on children likely would be attenuated.

To isolate variation in the severity of the recession driven by local labor demand, I construct an instrumental variable that predicts the 1978-1982 log employment change using a county's 1976 industrial structure and aggregate employment changes,

$$D_c^{78-82} = \sum_j \eta_{c,j,1976}(e_{-s(c),j,1982} - e_{-s(c),j,1978}). \tag{1.1}$$

In equation (1.1), $\eta_{c,j,1976}$ is the share of county c's employment in two-digit industry j in 1976, and $(e_{-s(c),j,1982} - e_{-s(c),j,1978})$ is the log employment change from 1978-1982 for industry j in all states in the same region besides the state of county c.[11] I interpret D_c^{78-82} as a shift to local labor demand for county c. This variable exploits the fact that the recession was more severe in counties that specialized in industries, like durable goods manufacturing or lumber products, that were more sensitive to fluctuations in interest rates, oil prices, or the business cycle.

Figure 1.3 shows that cross-county variation in the severity of the recession follows the predicted log employment change, as expected. A regression of the 1978-1982 log earnings per capita change on the predicted log employment change and state fixed effects, which I include in my baseline specification for estimating effects on individuals, implies that a 10 percent predicted employment decrease is associated with a 3.5 percent decrease in earnings per capita; when clustering standard errors by state, the F-statistic on this coefficient is 26.

Table 1.1 provides details on the aggregate patterns that underlie the predicted log employment change. The manufacturing sector lost 881,000 jobs from 1978-1982, and the construction sector lost 171,000 jobs. Within manufacturing, 546,000 jobs were lost in three industries alone: trans-

[11] There are 69 two-digit industries. I use the predicted log employment change because earnings data are not available at a sufficiently detailed industry level. Freeman (1980) uses a similar variable to study changes in employment across occupations, and many authors use this strategy to predict changes in local labor demand (e.g., Bartik, 1991; Blanchard and Katz, 1992; Bound and Holzer, 2000; Notowidigdo, 2013; Diamond, 2016). I exclude the contribution from a county's own state to remove a mechanical relationship between the actual and predicted change in economic activity. Using other states in the same region, as opposed to all other states, slightly improves explanatory power by allowing industry-level employment changes to differ by region.

portation equipment, primary metal (which includes steel mills), and lumber and wood products. Total employment increased by 4.5 million over this period, with notable growth in the mining sector (which includes oil and gas extraction) and the service sector.

1.2.3 The Evolution of Median Family Income from 1950-2000

To provide evidence on the validity of my empirical strategy, I next examine the relationship between the evolution of median family income from 1950-2000 and the severity of the recession predicted by pre-existing industrial specialization. My empirical strategy, which compares long-run outcomes of individuals born from 1950-1979, could confound the effect of the recession with pre-recession economic conditions if severe recession counties were on a downward trend from 1950-1980. In fact, I show that counties with a more severe recession saw greater income growth from 1950-1970, and this trend can be controlled for easily.

I examine the evolution of median family income from 1950-2000 by estimating the regression

$$y_{c,t} = \sum_{k=1950}^{2000} R_c^{78-82} 1(t = k)\alpha_k + x_{c,t}\beta + \gamma_c + \theta_{s(c),t} + \epsilon_{c,t}, \tag{1.2}$$

where $y_{c,t}$ is log real median family income in county c and year t.[12] The key explanatory variable is the 1978-1982 decrease in log real earnings per capita, R_c^{78-82}. In some specifications, $x_{c,t}$ contains time-varying covariates described below. The regression includes county fixed effects, γ_c, to absorb time-invariant differences across counties and state-by-year fixed effects, $\theta_{s(c),t}$, which I include in my baseline specification when estimating long-run effects on individuals. I normalize $\alpha_{1980} = 0$, so that $(\alpha_{1950}, \alpha_{1960}, \alpha_{1970})$ describe how the pre-recession evolution of log median family income is correlated with the severity of the 1980-1982 recession, and $(\alpha_{1990}, \alpha_{2000})$ describe the post-recession evolution. I estimate equation (1.2) with two stage least squares (2SLS), where the instrument for R_c^{78-82} is the predicted log employment change, D_c^{78-82}. I cluster standard errors

[12] I use median family income because it is available at the county-level from decennial censuses for 1950-2000 and is an important measure of local economic conditions. Unfortunately, county-level census data do not consistently report other quantiles of the family income distribution from 1950-2000. Earnings per capita, from the BEA, is only available for 1969-forward.

by state to allow for arbitrary serial and spatial correlation within states. I initially exclude the 526 counties with at least 5 percent of 1976 employment in the mining sector, which includes oil and gas extraction, to minimize the countercyclical boom-bust cycle in this sector. These high-mining counties account for only 6 percent of the U.S. population, but receive considerably more weight in 2SLS estimates of equation (1.2) because pre-existing industrial specialization strongly influences their earnings per capita.

The estimates of α_t in Figure 1.4 characterize the 1980-1982 recession as a reversal of post-war fortune: counties with a more severe recession saw greater median family income growth from 1950-1970. This pattern arises from estimates of model 1, which contains county and state-by-year fixed effects but no other covariates. Model 2, which adds an interaction between year and the 1950-1970 log median family income change, eliminates this pre-trend. The model 2 estimates imply that a 10 percent decrease in earnings per capita from 1978-1982 led to an 10 percent decrease in real median family income in 1990 and an 11 percent decrease in 2000.[13] In sum, the results in Figure 1.4 support the validity of my empirical strategy and underscore the lasting effect of the recession on local economic conditions.

1.2.4 Additional Results on the Cost of Housing and Government Expenditures

The effect of the recession on income could overstate the effect on purchasing power if the recession also reduced the cost of housing. Appendix A.3 provides evidence of a decline in median house values and rents from 1980-1990 in severe recession counties, but by less than the decline in median family income. As a result, the housing cost decline partly offset the income decline for renters, but led to a decrease in wealth for homeowners.[14]

Appendix A.4 examines the effects of the recession on local government expenditures and revenues, which could affect individuals' long-run education and income. Expenditures per capita

[13] When including the 526 counties with at least 5 percent of 1976 employment in mining, log median family income evolves similarly from 1950-1970, but does not decline after the recession (Appendix Figure A.6). Because relatively few people live in high mining counties, my estimates of long-run effects on children more closely reflect the persistence seen in Figure 1.4, which excludes high mining counties.

[14] Appendix A.3 also shows that commuting zones, which are aggregations of counties, also experience a persistent relative decrease in earnings per capita after the recession.

fell starting in 1992 in counties with a more severe recession, but there is little evidence of a decrease before then, likely due to higher federal transfers. The decline in expenditures is driven by spending on welfare and health, and not education. As a result, the decrease in local government expenditures might have had negative effects on individuals who were adolescents in the 1990's.

1.3 Possible Long-Run Effects of a Recession on Education and Income

This section draws on previous theoretical and empirical work to describe the possible long-run effects of a recession on education and income. Economic theory does not provide a sharp prediction about the magnitude or even sign of any long-run effects, but it does highlight potentially important channels.

First, a recession could affect educational attainment and lifetime income by decreasing human capital obtained during childhood. The stock of childhood human capital depends on a sequence of material and time investments from parents, a sequence of community investments from schools, neighborhoods, and peers, and an initial human capital endowment (Almond and Currie, 2011; Heckman and Mosso, 2014). A recession-induced decrease in the local wage could produce income and substitution effects. The income effect, due to a decrease in family earnings, predicts a decrease in parents' material investments.[15] The substitution effect, due to a decrease in the price of spending time with children, predicts an increase in parents' time investments, though this additional time might have limited benefits, or even be harmful, if the recession increases parental stress.[16] Community investments could fall due to a reduction in government expenditures or the quality of schools, neighborhoods, or peers.[17] I focus on individuals born before 1980, for whom

[15]Some studies find that children's long-run outcomes are sensitive to family resources (Aizer et al., 2016; Hoynes, Schanzenbach and Almond, 2016), while others do not (Jacob, Kapustin and Ludwig, 2015; Bleakley and Ferrie, 2016).

[16]Aguiar, Hurst and Karabarbounis (2013) show that parents spent more time with children during the Great Recession, and Del Boca, Flinn and Wiswall (2014) find that parental time produces cognitive skills in children. On economic shocks and parenting behavior, see McLoyd et al. (1994); Leininger and Kalil (2014); Akee et al. (2015); Brand (2015).

[17]Existing work documents spillover effects of disruptive peers (Figlio, 2007; Carrell and Hoekstra, 2010; Carrell, Hoekstra and Kuka, 2016) and finds that neighborhoods affect children's long-run outcomes (Chetty and Hendren, 2016*a*; Chetty, Hendren and Katz, 2016; Chyn, 2016).

the recession does not affect initial human capital endowments.[18]

A recession also could affect educational attainment and lifetime income by shaping the decision to finish high school or obtain a college degree. In choosing their desired level of schooling, individuals trade off higher lifetime earnings against the opportunity cost of forgone earnings and the cost of tuition (Mincer, 1958; Becker, 1962; Ben-Porath, 1967). A recession could reduce the opportunity cost by reducing the earnings of less educated workers, leading to long-run increases in education and income.[19] However, in the presence of credit constraints, a recession might decrease parents' ability to pay for tuition, leading to long-run decreases in education and income.[20]

The conceptual framework informs the unit of geography I use to measure recession exposure in my empirical analysis. A recession could have long-run effects on individuals because of mediating effects on parents, schools, neighborhoods, peers, and the local labor market (through the opportunity cost of schooling). Unfortunately, I am unable to link individuals to their parents, school district, neighborhood, or peer group, and data do not exist that measure the severity of the recession for these groups.

The most detailed unit of geography in my data is county of birth. While counties do not map exactly to school districts, neighborhoods, or peer groups, they resemble these sources of local community investments more closely than do commuting zones or other aggregations of counties. One possible concern with using counties is that they inadequately reflect local labor market opportunities. However, the BEA data report earnings by county of *residence*, and so reflect individuals' commuting throughout the local labor market without imposing the assumption

[18]Previous work finds that infant health improves with the unemployment rate (Dehejia and Lleras-Muney, 2004) and recession-driven reductions in air pollution (Chay and Greenstone, 2003). My results are not affected by selective fertility or *in utero* recession exposure, which are the primary mechanisms considered by these authors.

[19]Empirical work finds an important role for opportunity cost in educational attainment (Black, McKinnish and Sanders, 2005; Cascio and Narayan, 2015; Charles, Hurst and Notowidigdo, 2015; Atkin, 2016). Bound and Holzer (2000) and Hoynes, Miller and Schaller (2012) show that the 1980-1982 recession especially reduced the wages and employment of less educated workers.

[20]Several studies conclude that short-term credit constraints are relatively unimportant for college attendance or graduation (Cameron and Heckman, 2001; Cameron and Taber, 2004; Stinebrickner and Stinebrickner, 2008), but evidence from college decisions made in the early 2000's suggests a larger role for credit constraints (Belley and Lochner, 2007; Bailey and Dynarski, 2011; Lovenheim, 2011). Charles, Hurst and Notowidigdo (2015) find that the early 2000's housing boom decreased college enrollment, consistent with opportunity cost being more important than parental resources in their setting.

that commuting zones perfectly capture commuting patterns. If individuals' perceptions of labor market opportunities depend especially on the experiences of individuals who live nearby, then earnings per capita for residents in the same county is more meaningful than earnings per capita throughout the local labor market. These considerations suggest that counties, although imperfect, are preferable to commuting zones. Another advantage of using counties is that birth county fixed effects more flexibly control for time-invariant components of initial human capital endowments, parents, and communities that shape long-run outcomes. I examine the sensitivity of my results to measuring exposure to the recession using individuals' county of birth, commuting zone of birth, and a weighted average of nearby counties. Results using these different units of geography are broadly consistent (for comparisons see Appendix A.9).

1.4 Data and Empirical Strategy

1.4.1 Data on Long-Run Outcomes and County of Birth

To estimate the long-run effects of the 1980-1982 recession, I link the 2000 Census and 2001-2013 American Community Surveys to the Social Security Administration NUMIDENT file. The resulting data contain adult outcomes and county of birth for millions of people. My sample consists of individuals born in the U.S. from 1950-1979 who were age 25-64 at the time of the survey. I exclude individuals living in group quarters, who are not in the 2001-2005 ACS data, and individuals with imputed values of age, sex, race, or state of birth. I also exclude individuals with imputed dependent variables, leading to three nested samples. My first sample contains 23.5 million individuals with non-imputed years of education. My second sample contains 18.4 million individuals that also have non-imputed labor market outcome variables, and my third sample contains 15.6 million individuals that also have positive personal income, family income, and hourly wage. These samples balance the goals of using as much information as possible, given non-trivial imputation rates for labor market outcomes, and limiting the number of samples to ensure that no confidential information is disclosed.[21] I limit the sample to the 89 percent of individuals with a unique PIK, which is an anonymized identifier, and unique birth county.[22]

1.4.2 Difference-in-Differences Specification using Pre-Existing Industrial Structure and the 1980-1982 Recession

I estimate the long-run effects of the 1980-1982 recession with a generalized difference-in-differences framework that compares education and income in adulthood of individuals born in counties with a more versus less severe recession (first difference) and individuals who were younger versus older when the recession began (second difference). I use 2SLS to isolate variation in the severity of the recession driven by the interaction of a county's pre-existing industrial specialization and aggregate employment changes in certain industries following the increase in interest rates, the strengthening of the U.S. dollar, high oil prices, and the decline in aggregate demand.

In particular, consider the individual-level regression model

$$y_{i,a,c,t} = \sum_k R_c^{78-82} 1(a = k)\pi_k + x_{i,a,c,t}\beta + \gamma_c + \theta_{a,s(c)} + \delta_t + \varepsilon_{i,a,c,t}, \tag{1.3}$$

where $y_{i,a,c,t}$ is a measure of educational attainment or income in adulthood of individual i, who was age a in 1979, born in county c, and observed in survey year t. The explanatory variable of

[21] In publicly available 2000-2013 Census/ACS data, for people born in the U.S. from 1950-1979 who were age 25-64 at the time of the survey, 8.1 percent of individuals have imputed values of age, sex, race, or state of birth, with state of birth being imputed most frequently. Among individuals with no imputations in these basic demographic variables, 1.8 percent are in group quarters. A further 1.1 percent of individuals have imputed years of education, and 21.3 percent have imputed education or labor market variables (any of the seven individual income variables, total family income, weeks worked, hours worked, marital status, and labor force status). I limit the sample to individuals who tell the Census Bureau that they were born in the U.S. to reduce false birth county matches.

[22] The Census Bureau assigns Protected Identification Keys (PIKs) to individuals in the Census and ACS using information on respondents' name, address, date of birth, and gender. Sometimes a PIK is assigned to more than one respondent in a survey year. While technically possible (e.g., if an individual receives a survey at multiple residences), this outcome likely reflects an error in PIK assignment. An individual may be assigned to multiple birth counties if the 12-character string from the NUMIDENT does not identify a single county. For example, there are two towns named Arcadia in North Carolina, and a respondent who writes "Arcadia" could be matched to two counties. Appendix A.5 describes the algorithm that identifies individuals' county of birth using the 12-character string for place of birth from the NUMIDENT, which comes from Social Security card applications. The algorithm was developed alongside Martha Bailey, Evan Taylor, and Reed Walker. For researchers in a secure Census Bureau computing environment, we provide additional detail in Taylor, Stuart and Bailey (2016).

interest is R_c^{78-82}, which measures recession severity as the decrease in log real earnings per capita from 1978-1982 in county c. I use the predicted log employment change from 1978-1982, D_c^{78-82}, as an instrumental variable for R_c^{78-82} (see Figure 1.3). The vector $x_{i,a,c,t}$ includes indicators for sex and race, a cubic in age at the time of the survey to capture life-cycle patterns, and interactions between age in 1979 and the 1950-1970 change in log median family income to control for the pre-recession trend in economic activity (see Figure 1.4). Birth county fixed effects, γ_c, absorb cross-county differences in initial human capital endowments, as discussed in Section 1.3, plus fixed characteristics of parents and communities. Age in 1979-by-birth state fixed effects, $\theta_{a,s(c)}$, control for changes over time in state-level higher education access, transfer programs, and other factors.[23] Survey year fixed effects, δ_t, absorb differences across survey years.

The parameter of interest, π_a, measures the effect of the recession on individuals who were age a in 1979. Section 1.2 shows that 1980-1982 recession led to a persistent decline in local economic activity, and π_a reflects this persistence. I allow π_a to vary flexibly with age in 1979 because the operative mechanisms, sensitivity to the recession, and stock of childhood human capital might vary with individuals' age when the recession begins. I normalize the parameter for individuals age 29 in 1979, $\pi_{29} = 0$, so the identified parameters can be interpreted as the effect for individuals age a in 1979 minus the effect for 29 year olds, $\pi_a - \pi_{29}$. For education outcomes, 29 year olds provide a useful comparison group because they mostly completed their schooling before the recession. Individuals between the ages of 23-28 also mostly completed their schooling before the recession, which suggests a placebo test of whether $\pi_a = 0$ for $a = 23, \ldots, 28$.[24] For income, this approach could yield estimates that are biased upwards if 29 year olds also experienced a lasting decrease in income (i.e., $\pi_{29} < 0$) because of job loss (Davis and von Wachter, 2011) or a decline

[23]The determinants of higher education access include cohort size, state appropriations, tuition, and financial aid. Given balanced budget requirements, higher education appropriations tend to fall when tax revenue falls or expenditures on other, less flexible items rise (Kane, Orszag and Apostolov, 2005; Delaney and Doyle, 2011). Appropriations and tuition depend on a variety of factors besides the economy, including state politics and institutional features such as who sets tuition (Kane, Orszag and Apostolov, 2005; Doyle, 2012). Given the conceptual and measurement challenges involved with controlling for non-recession related changes in higher education access, transfer programs, and other factors, my preferred specification includes age in 1979-by-birth state fixed effects.

[24]For individuals born from 1957-1964, about 75 percent of four-year college degree attainment is completed by age 25 and 85 percent is completed by age 29 (see Appendix Figure A.10).

in local job quality (Hagedorn and Manovskii, 2013; Kahn and McEntarfer, 2015). Because I find negative effects on income, this suggests that my estimates might be too conservative.

To reduce computational burden, I collapse Census and ACS individual-level data into cells defined by age in 1979, birth county, survey year, race, and sex, and I estimate grouped regressions with weights given by the number of observations in each cell. This grouped regression produces point estimates that are nearly identical to those from an individual-level regression.[25] I cluster standard errors by birth state to allow for arbitrary serial and spatial correlation within states.

1.4.3 Addressing Measurement Error in Recession Exposure

Estimates of π_a are likely biased towards zero because some individuals' pre-recession location differed from their county of birth, which is all I observe. To see this, let $\tilde{R}_{a,c}^{78-82}$ be the true average exposure to the recession for individuals who were age a in 1979 and born in county c. As I define it, true recession exposure depends on where individuals lived in 1979, but does not depend on post-recession migration, which is one of many actions that parents might take to mitigate the effects of the recession.

The extent of measurement error is summarized by the equation

$$\tilde{R}_{a,c}^{78-82} = \sum_k R_c^{78-82} 1(a = k)\lambda_k + x_{a,c}\tilde{\beta} + \tilde{\theta}_{a,s(c)} + v_{a,c}, \tag{1.4}$$

where $x_{a,c}$ contains the share of individuals who are female and non-white (corresponding to the sex and race indicators in equation (1.3)), plus interactions between age in 1979 and the 1950-1970 change in log median family income.[26] If unobserved measurement error, $v_{a,c}$, is uncorrelated with unobserved determinants of long-run outcomes, $\varepsilon_{i,a,c,t}$, conditional on the covariates in equations (1.3) and (1.4), then plim $\hat{\pi}_a = \tilde{\pi}_a \lambda_a$, where $\hat{\pi}_a$ is the OLS or 2SLS estimate of the effect of

[25] If I used indicator variables instead of a cubic for age, the grouped regression would produce identical point estimates as the individual-level regression. Isen, Rossin-Slater and Walker (Forthcoming) also use a grouped regression to reduce computational burden.

[26] Equation (1.4) does not contain the cubic in age, survey year fixed effects, or birth county fixed effects that are in equation (1.3). I do not include birth county fixed effects in equation (1.4) because the attenuation bias arises from cross-county variation. Furthermore, including a birth county fixed effect, say $\tilde{\gamma}_c$, in equation (1.4) is not feasible: this would yield a term $\tilde{\gamma}_c 1(a = k)$ which is collinear with $R_c^{78-82} 1(a = k)$.

R_c^{78-82} from equation (1.3), and $\tilde{\pi}_a$ is the effect of true average recession exposure, $\tilde{R}_{a,c}^{78-82}$.[27] Consequently, the estimated effects of the recession will be attenuated if $\lambda_a \in (0, 1)$, and I can eliminate this attenuation bias with an estimate of λ_a.

To quantify the extent of attenuation, I estimate λ_a using two data sets that provide valuable, but imperfect, information.[28] First, I use 2000-2013 Census/ACS data for individuals born from 1990-2013. These data contain county of birth from the NUMIDENT, like the data I use to estimate the long-run effects of the 1980-1982 recession, and county of residence. However, they measure the relationship between county of birth and county of residence after the 1980-1982 recession and might not accurately characterize the relevant measurement error if family migration patterns have changed over time. To address this limitation, I use confidential Panel Study of Income Dynamics (PSID) data. PSID data allow me to estimate λ_a for individuals born from 1968-1979 and observed in 1979, but they only contain information on county of residence.[29] As seen in Appendix Figure A.11, point estimates of λ_a from Census/ACS data range from 0.75 for 0 year olds to 0.58 for 17 year olds. Point estimates from PSID data display a similar age profile, but are larger because county of residence is more strongly related to county of residence in early life than county of birth. Appendix Figure A.11 strongly suggests that my estimates of the recession's long-run effects are attenuated. These estimates are consistent with a lack of anticipatory migration before the recession on average. If families anticipated the severity of the recession in local areas and moved to areas where the recession would be less severe, then estimates of λ_a could be negative.

Below, I adjust for this attenuation using the Census/ACS data because they contain the relevant information on place of birth. The validity of this adjustment depends on two assumptions. First, I assume that unobserved measurement error, $v_{a,c}$, is uncorrelated with unobserved determinants of long-run outcomes, $\varepsilon_{i,a,c,t}$, conditional on the covariates in equations (1.3) and (1.4). For example, this rules out the possibility that families with young children from counties with high observed determinants of long-run outcomes, $\varepsilon_{i,a,c,t}$, anticipated the recession and moved to less severe

[27] See Bound et al. (1994) for an in-depth discussion of the consequences of measurement error.

[28] The ideal data set is the 1980 Census linked to the NUMIDENT. Unfortunately, these files are not currently linked.

[29] I limit the PSID sample to individuals who are first observed before age 3 to make early life county of residence a better proxy for county of birth.

recession counties before 1980. The suddenness of changes in local economic activity provide some support for this assumption. Second, I assume that estimates of λ_a for individuals born from 1990-2013 accurately characterize the measurement error relationship for individuals born from 1950-1979. While I cannot test this assumption directly, support for it comes from the fact that estimates of λ_a are stable across the 1968-2013 birth cohorts in the PSID (Appendix Table A.12).[30]

1.4.4 Potential Threats to Empirical Strategy

One threat to my empirical strategy is that, even in the absence of the 1980-1982 recession, long-run outcomes of individuals born in counties with a more severe recession would have evolved differently across cohorts than for individuals born in the same state in counties with a less severe recession. A confounding variable would have to be orthogonal to the pre-recession evolution of log median family income, for which I control. Because I estimate equation (1.3) with 2SLS, the relevant measure of recession severity is the predicted log employment change from 1978-1982. For example, a relative decline in infant health from 1950-1979 in counties with a larger predicted log employment decrease would threaten my strategy.

Several pieces of evidence suggest that this potential threat is unimportant. My empirical strategy exploits sharp changes in local economic activity driven by the interaction of pre-existing industrial specialization and aggregate employment changes that emerged during the 1980-1982 recession. This design mitigates many potential concerns about confounding selective migration or fertility before 1980. As shown in Figure 1.4, the type of industrial specialization that hurt counties during the 1980-1982 recession was uncorrelated with median family income growth from 1970-1980 and positively correlated with income growth from 1950-1970, for which I control. Furthermore, there is little correlation between the predicted log employment change from

[30]My adjustment divides point estimates $\hat{\pi}_a$ by $\hat{\lambda}_a$. I use the estimate of λ_a for 17 year olds in Appendix Figure A.11 for individuals 18 and older in 1979. Migration rates increase after age 17, as children leave their parents' household, but parents' location seems most relevant for educational attainment. Because the unadjusted estimates of π_a are close to zero for individuals age 18 and older in 1979, the adjusted estimates will be small regardless of the specific approach. As additional information, Appendix Figure A.12 displays the share of individuals born from 1990-2013 that are living outside of their county, commuting zone, and state of birth. Appendix Figure A.13 compares the share of individuals living outside their birth county using the Census/ACS and PSID data. Appendix Figure A.14 shows that migration rates are remarkably stable across the 1968-2013 birth cohorts in the PSID.

1978-1982 and the severity of other economic shocks arising from recessions or Chinese import competition.[31]

To provide direct evidence on the pre-recession evolution of infant health and parental characteristics, I use county-level birth certificate data to estimate 2SLS regressions similar to equation (1.2). As detailed in Appendix A.7, there is no evidence that the evolution of infant mortality from 1950-1979 is correlated with the severity of the 1980-1982 recession. There is also no evidence of a relationship between the severity of the recession and the 1970-1979 evolution of maternal education, various measures of low birth weight, or median birth weight.

1.5 The Long-Run Effects of the Recession on Education

Before discussing the estimated long-run effects of the recession on education, I describe the effects predicted by potentially important economic channels. Figure 1.5 plots hypothesized effects of the recession on college degree attainment, corresponding to the parameter π_a in equation (1.3). I model the recession as a one-time, persistent decrease in local labor demand, which is consistent with the evidence in Section 1.2. I assume that individuals decide whether to attend college at age 18 and, if they graduate, do so at age 22. As a result, the recession does not affect college degree attainment for individuals who are 22 or older in 1979. If the recession reduces childhood human capital, several reasons suggest that effects will be more severe for younger children.[32] The opportunity cost channel predicts a positive effect on college degree attainment, and the parental resources for college channel predicts a negative effect in the presence of credit constraints.[33] Par-

[31] Appendix Table A.11 shows negligible within-state correlations between the change in log real earnings per capita from 1978-1982 and during other recessions, and negligible within-state correlations between the predicted log employment change from 1978-1982 and the log earnings per capita change during other recessions. As described in Appendix A.3, there is little correlation between the log earnings per capita change or predicted log employment change from 1978-1982 and exposure to Chinese import competition as measured by Autor, Dorn and Hanson (2013).

[32] First, a persistent recession could lead to several periods of reduced investment, with more severe cumulative effects for younger children. Second, the human capital production function might feature dynamic complementarity, so that less investment in early childhood reduces the return to investment in later childhood (Cunha and Heckman, 2007; Cunha, Heckman and Schennach, 2010; Aizer and Cunha, 2012; Caucutt and Lochner, 2012). Finally, early childhood might be a critical or sensitive period (Almond and Currie, 2011; Heckman and Mosso, 2014).

[33] If parents of older children had more savings, then the negative effects under the parental resources channel would be more severe for younger children.

ents could also mitigate these effects by moving away from severe recession counties if there are no associated disruption costs. My empirical strategy measures the net long-run effects, which might depend on all of these channels. Figure 1.5 plots stylized age-effect profiles, but my regressions do not impose these restrictions.

Figure 1.6 shows that the 1980-1982 recession led to a long-run reduction in four-year college degree attainment. The figure displays 2SLS estimates of equation (1.3), where I use three-year age bins to estimate π_a more precisely.[34] The estimates for individuals who were 23-28 years old in 1979 are small and indistinguishable from zero ($p = 0.52$), indicating that the severity of the recession is not related to college degree attainment for this group. Because college degree attainment is mostly completed by age 23, this finding supports the validity of my empirical strategy. Negative effects gradually emerge for individuals who were younger when the recession began; effects are most severe, essentially constant, and statistically significant for individuals age 0-13 in 1979. For this group, the point estimates imply that a decrease in earnings per capita from 1978-1982 of 10 percent, which is slightly smaller than one standard deviation, reduces four-year degree attainment by three percentage points, or around nine percent of mean attainment. As seen by comparing these estimates to the hypothesized effects in Figure 1.5, the negative effects likely stem from a decrease in childhood human capital development or a decrease in parental resources to finance college in the presence of credit constraints. The small and insignificant effects for individuals age 14-19 in 1979 are not consistent with the simplest model of credit constraints, which predicts negative effects for this group because parents immediately face challenges in paying for college.

Figure 1.6 also shows the consequences of adjusting for attenuation bias due to individuals' pre-recession location differing from their county of birth. The adjusted effects are larger in magnitude, but the age profiles of the unadjusted and adjusted effects are reasonably similar. In the rest of the paper, I focus on the conservative estimates that do not adjust for pre-recession migration.

Table 1.2 reports estimates of the long-run effects of the recession on several measures of educational attainment, grouping together individuals age 0-10, 11-19, and 20-28 in 1979. There

[34]The bins are for 1979 ages 0-1 (born in 1978-1979), 2-4, 5-7, 8-10, 11-13, 14-16, 17-19, 20-22, 23-25, and 26-28.

is no evidence of an effect on the attainment of at least a high school diploma or GED. The point estimates for college attendance are negative, but small and indistinguishable from zero. Column 3 shows sizable and statistically significant negative effects on any college degree attainment for individuals age 0-19 in 1979. For individuals age 11-19, the effects on college attendance and degree attainment are similar in magnitude. For individuals age 0-10, the recession reduces degree attainment more than attendance, suggesting a decrease in college persistence. Columns 4 and 5 separate college degree attainment into two mutually exclusive categories: four- and two-year degree attainment.[35] There is evidence of an increase in two-year degree attainment for individuals age 0-10 in 1979, consistent with substitution from four- to two-year colleges. For this group, a 10 percent decrease in earnings per capita from 1978-1982 leads to a 1.8 percentage point (4.4 percent) decrease in any college degree attainment, a 3.0 percentage point (9.4 percent) decrease in four-year degree attainment, and a 1.2 percentage point (12.8 percent) increase in two-year degree attainment. Overall, the negative effects of the recession are concentrated at higher levels of educational attainment, for which childhood human capital and parental resources seem most valuable (Belley and Lochner, 2007; Bailey and Dynarski, 2011).

By way of providing some context, the effect of the 1980-1982 recession on any college degree attainment for individuals age 0-10 in 1979 is comparable in magnitude to Project STAR, which reduced class sizes from kindergarten to grade 3 and increased college degree attainment by 1.6 percentage points (Dynarski, Hyman and Schanzenbach, 2013). The effect of the recession on four-year college degree attainment is larger than Project STAR, which increased four-year degree attainment by 0.9 percentage points (Dynarski, Hyman and Schanzenbach, 2013), and is comparable in magnitude to statewide scholarship programs that offered free tuition and fees for qualified students, which increased four-year degree attainment by 3 percentage points (Dynarski, 2008).

Several studies find that improved local labor market opportunities reduce high school and college enrollment, as predicted by the opportunity cost channel (Black, McKinnish and Sanders, 2005; Cascio and Narayan, 2015; Charles, Hurst and Notowidigdo, 2015; Atkin, 2016). Most

[35] Census and ACS data measure the highest degree completed, so an individual with a two- and four-year degree is coded as having a four-year degree.

directly, this channel predicts positive effects of the recession on high school graduation and college attendance for individuals age 14-19 in 1979. After adjusting for pre-recession migration, my point estimates are somewhat smaller than those in prior work, but confidence intervals admit similar results.[36]

While the recession could affect children whose parents do not lose their job, the long-run effects of parental job displacement provide another benchmark. If job loss generated all of the county-level decrease in earnings per capita from 1978-1982 and the recession only affected children whose parents lost a job, the estimates in Page, Stevens and Lindo (2007), Coelli (2011), and Hilger (2016) suggest that a 10 percent decrease in earnings per capita would decrease college enrollment by 0.5-10 percentage points.[37] The estimates in Table 1.2 lie within this range, but this comparison provides limited information on how much of the effect of the recession stems from parental job loss.

Appendix Table A.14 reports OLS and reduced-form estimates, and Appendix Table A.15 reports first stage estimates.[38] Appendix A.8 describes results that attempt to separate the effects of temporary and persistent decreases in earnings per capita that emerged with the onset of the 1980-

[36]As shown in Appendix Figures A.16 and A.17, the upper ranges of 95 percent confidence intervals indicate that a 10 percent decrease in earnings per capita from 1978-1982 leads to no more than a 1.7 percentage point (1.8 percent) increase in high school graduation for individuals age 14-16 in 1979 and a 1.6 percentage point (3.0 percent) increase in college attendance for individuals age 17-19 in 1979. Black, McKinnish and Sanders (2005), who study the coal boom and bust in the 1970's and 1980's in Appalachia, find that a 10 percent decrease in earnings per worker leads to a 4.4-7.2 percent increase in high school enrollment. Cascio and Narayan (2015), who study the fracking boom in the 2000's in mainly rural U.S., find that a 10 percent decrease in the high school wage premium leads to a 4.7 percent increase in high school enrollment. Charles, Hurst and Notowidigdo (2015), who study the housing boom in the 2000's, find that a 10 percent increase in log wages of adults age 18-25 is associated with a 1.8 percent decrease in college attendance.

[37]Using the PSID, Page, Stevens and Lindo (2007) estimate a negative but insignificant effect of parental job displacement, due to firm closure, on college attendance. Studying job displacements in Canada in the 1980's, Coelli (2011) finds that parental job displacement reduces college enrollment by 10 percentage points. Hilger (2016) studies job displacements in the U.S. from 2000-2009 and finds a reduction in college enrollment of 0.5 percentage points. These papers find that job displacement reduces long-run family income by around 10 percent.

[38]The OLS estimates typically are attenuated relative to the 2SLS estimates. One explanation is that a labor demand shock has more severe effects than a labor supply shock, and the OLS estimates reflect labor supply shocks more than the 2SLS estimates. A related explanation is that the local average treatment effect of an earnings decrease due to a change in the number of jobs is larger than the effect of a general earnings decrease. A third possible explanation is that the 2SLS estimates reduce attenuation bias from measurement error in the 1978-1982 decrease in log earnings per capita (see also Charles and Stephens, 2013). Measurement error could arise when the BEA converts earnings reported by place of work to place of residence; this is distinct from the measurement error that arises due to individuals' pre-recession location differing from their place of birth. First stage slope coefficients are centered around 0.5, with F-statistics between 18 and 20.

1982 recession. As detailed in Appendix A.9, my results are robust to different sets of covariates and different measures of recession severity.

1.5.1 Heterogeneity by Sex and Race

To better understand who was affected by the recession, I separately estimate the long-run effects on men and women. Men experienced a greater decline in employment and wages during the recession (Bound and Holzer, 2000; Hoynes, Miller and Schaller, 2012), and if this pattern persisted, the opportunity cost channel would predict a greater increase in educational attainment for them.[39] Because men and women grew up in the same families and neighborhoods, they were likely exposed to similar conditions in childhood and had similar levels of parental resources when deciding whether to attend college. However, exposure to the recession during childhood might have different effects on men and women. Recent work finds that disadvantage in childhood has more severe effects on the long-run outcomes of men than women (Autor et al., 2016; Chetty et al., 2016), while other papers find that women are equally or more sensitive to childhood disadvantage (Chetty, Hendren and Katz, 2016; Chyn, 2016).

Panel A of Table 1.3 shows that the recession reduced long-run educational attainment of both men and women. The recession had more severe effects on college attendance and any college degree attainment of men, consistent with higher sensitivity to early life disadvantage for men. The recession had more severe effects on the four-year college degree attainment of women, and this appears to be driven by greater substitution among women from four- to two-year colleges. One explanation for this differential substitution is a higher return to two-year degrees for women (Kane and Rouse, 1995; Jepsen, Troske and Coomes, 2012).

I also separately estimate the long-run effects of the recession on whites and non-whites. While non-white workers experienced greater reductions in employment and wages during the recession (Bound and Holzer, 2000; Hoynes, Miller and Schaller, 2012), it is unclear whether this differential persisted. If it did, the opportunity cost channel would predict a greater increase in educational

[39]Nonetheless, the longer-run effects could differ by sex, and it would be interesting to study this directly.

attainment for non-whites. However, non-whites also might have experienced greater reductions in childhood human capital and parental resources to pay for college, which predicts a greater decrease in education for non-whites. Another possibility is that non-whites are less likely to be on the margin of obtaining a college degree because they face higher levels of disadvantage.[40] Panel B of Table 1.3 shows that the negative effects of the recession on educational attainment are concentrated among whites. For non-whites, there is evidence of an increase in high school graduation and college attendance, but little evidence of an effect on college degree attainment.

1.5.2 Heterogeneity by Features of Birth State and County

Next, I describe regressions that provide evidence on the underlying mechanisms and policies that might mitigate the recession's long-run effects. In particular, I examine interactions between the effect of the recession and features of individuals' birth state and county. This augments my baseline specification in equation (1.3), which controls for time-varying state factors and time-invariant county factors, but does not include interactions with the severity of the recession. I focus on four-year college degree attainment because of its importance for individuals and the economy.

The effect of a decline in local economic activity could be stronger in states with a more severe recession if they had less capacity to direct transfers to negatively affected counties or offered fewer opportunities for parents seeking to migrate or commute to stronger labor markets. To examine this possibility, columns 1 and 2 of Table 1.4 divide the sample into states with a more and less severe recession, where states with a more severe recession had an above-median decrease in log earnings per capita from 1978-1982.[41] The negative point estimates are only half as large in more severe recession states, but the estimates are not statistically distinguishable ($p = 0.62$). These results provide little evidence that the negative effects of the recession are exacerbated by effects on state public finances or mitigated by nearby migration or commuting opportunities.

[40] In publicly available 2000-2013 Census/ACS data, 32.4 percent of whites and 19.9 percent of non-whites born from 1950-1979 attained a four-year college degree.

[41] Appendix Table A.23 characterizes this and other dimensions of state-level heterogeneity.

States might have mitigated the recession's long-run effects with more generous transfer programs, which could insure households and communities against earnings declines. In measuring the generosity of a state's transfer program, it is important to control for economic and demographic characteristics that are mechanically related to higher transfer expenditures. I attempt to do so by regressing, at the state-level, log transfers per capita in 1970 on log median family income in 1969 and the share of the 1970 population that is black, female, foreign born, urban, a high school graduate, a college graduate, age 5-19, age 20-64, and age 65 and above.[42] Columns 3 and 4 of Table 1.4 divide the sample into states with more and less generous transfers per capita using the residuals from this regression. For individuals age 0-10 in 1979, the effect of the recession is 30 percent less severe in states with more generous transfers. However, the estimates are not statistically distinguishable ($p = 0.50$). As a result, there is little evidence that states with more generous transfer programs mitigated the recession's effects.

Another possibility is that the effect of the recession was diminished in states that tended to transfer more money to poorer counties. To characterize states' transfer progressivity, I regress log transfers per capita in 1970 on log median family income in 1969, state fixed effects, and the previously described control variables, with the dependent and explanatory variables measured at the county-level. Columns 5 and 6 present results from dividing states into two groups using the state-specific slope coefficient on log median family income.[43] The effects of the recession are considerably less severe in states with more progressive transfers. However, the estimates are not statistically distinguishable ($p = 0.96$), so there is little evidence that states with more progressive transfers mitigated the recession's effects.

Benchmark models predict that a negative shock to childhood human capital will have more severe consequences for children with lower levels of initial human capital because the marginal product of investment is larger at lower levels of human capital (Almond and Currie, 2011; Heckman and Mosso, 2014). Consequently, a recession might have more severe effects in counties with

[42] I focus on transfers over which states have some control: retirement and disability insurance (excluding Social Security), Medicare, public assistance medical care benefits (primarily Medicaid), income maintenance benefits (including SSI, Food Stamps, and AFDC), unemployment insurance compensation, and education and training assistance.

[43] Card and Payne (2002) use a similar approach to characterize state-level school aid systems.

higher rates of pre-recession poverty.[44] Columns 7 and 8 divide the sample into counties with a higher versus lower poverty rate in 1970. The effects are more severe in high poverty counties, as predicted, but are not statistically different ($p = 1.00$). These results provide little evidence that the recession exacerbated initial levels of disadvantage.

The comparisons in Table 1.4 examine whether the long-run effects of the recession were amplified by the severity of the recession at the state-level and whether they were mitigated by state transfer programs. The comparisons also examine whether the recession deepened initial levels of disadvantage. While point estimates suggest that the effects of the recession were more severe in states with less generous transfers, in states with less progressive transfers, and in counties with higher initial levels of poverty, all of the comparisons are statistically indistinguishable. More research is needed to understand the factors that could exacerbate or mitigate the long-run effects on education.

1.6 The Long-Run Effects of the Recession on Income, Wages, and Poverty

The previous section shows that the 1980-1982 recession led to a sizable long-run decrease in college degree attainment. Standard models of worker productivity suggest that these reductions in schooling should lower lifetime earnings and could increase economic disadvantage. To provide evidence on this, I next examine the long-run effects on income, wages, and poverty.

Table 1.5 shows that the recession led to a lasting decrease in income and wages and a lasting increase in poverty. For individuals age 0-10 in 1979, the estimates imply that a 10 percent decrease in earnings per capita from 1978-1982 reduces personal income by 2.2 percent ($926), earned income by 3.2 percent ($1,314), and hourly wages by 1.8 percent ($0.45).[45] For the same group, total family income falls by 3.7 percent ($2,961), and the probability of living in poverty

[44] Other channels, such as parental resources to pay for college, could also lead to more severe effects in high poverty counties.

[45] Personal income is the sum of wage and salary, business and farm, welfare, Social Security, Supplementary Security, investment, retirement, and other income. Earned income is wage and salary plus business and farm income. To limit the influence of potential outliers in the self-reported income data, for each income category I replace values above the 99.5th percentile in each survey year-by-state of residence cell with the average among those above the 99.5th percentile. I construct total and earned income as the sum of the non-imputed, top-code-adjusted components.

increases by 13.9 percent (1.7 percentage points).[46] Individuals who were age 11-19 in 1979 experience a similar decrease in income and wages and a slightly smaller increase in poverty.[47] These results indicate that the recession led to a sizable long-run decrease in individuals' labor market productivity and an increase in economic disadvantage. Because the recession might have negatively affected individuals who were 29 years old in 1979, these estimates could be biased upwards (i.e., too conservative).

The age-effect profiles in Table 1.5 differ somewhat from Table 1.2, which shows more severe effects on college degree attainment for individuals who were younger when the recession began. One likely explanation is life-cycle bias due to my inability to observe all individuals at the same age (Haider and Solon, 2006). In particular, the effect of the recession on income early in an individual's career (i.e., for someone who was younger when the recession began) could be biased upwards relative to the effect on lifetime income.[48] Another possible explanation is that the recession reduces the non-cognitive skills of adolescents, and that this decline in non-cognitive skills reduces income more so than education (Heckman, Stixrud and Urzua, 2006; Akee et al.,

[46] In constructing family income and poverty rates, I separate extended families living in the same household (see Hoynes, Page and Stevens, 2006). I construct family interrelationship variables in the confidential Census/ACS data using code that almost exactly matches the variables constructed by Ruggles et al. (2015) in IPUMS data.

[47] For completeness, Appendix Tables A.24 and A.25 present results for other outcomes, which are harder to interpret given the possibility that the recession affected individuals who were 29 years old in 1979. In Table 1.5, the effect of the recession on log family income is similar for individuals age 0-10 and 11-19 in 1979, but the effect on poverty is twice as large for individuals age 0-10; this is largely explained by the fact that individuals who were 0-10 years old in 1979 have slightly larger families, likely due to an increased propensity to live with their parents from 2000-2013.

[48] To see the role of life-cycle bias in my difference-in-differences model, suppose I only observe income in year 2000 for individuals born in 1950 and 1975. I divide counties into more (m) and less (ℓ) severe recession counties. After partialling out covariates, the OLS difference-in-differences estimate for people who were 4 years old in 1979 is

$$\hat{\pi}_4 = \left(y^{25}_{1975,m} - y^{25}_{1975,\ell}\right) - \left(y^{50}_{1950,m} - y^{50}_{1950,\ell}\right),$$

where $y^{25}_{1975,m}$ is mean (residualized) income for individuals born in 1975 in a more severe recession county who are observed at age 25. Because the recession reduces college degree attainment, the early career income difference between individuals from a more versus less severe recession county is likely less negative than the lifetime income difference, so that $(y^{25}_{1975,m} - y^{25}_{1975,\ell}) > (\bar{y}_{1975,m} - \bar{y}_{1975,\ell})$, where $\bar{y}$ is mean lifetime income. In addition, the late career income difference between individuals from a more versus less severe recession county could be more negative than the lifetime income difference (e.g., because early career income is earned before the recession), so that $(y^{50}_{1950,m} - y^{50}_{1950,\ell}) < (\bar{y}_{1950,m} - \bar{y}_{1950,\ell})$. Life-cycle bias is similar when estimating the model by 2SLS. Consequently, both early and late career life-cycle bias could lead to an upwards bias in the difference-in-differences estimates. The estimates in Haider and Solon (2006) suggest substantial life-cycle bias up to age 30. Because the 2000 Census has more observations than the 2001-2013 ACS samples, my estimates place higher weight on earlier ages, which suggests that life-cycle bias could be quantitatively important.

2015). Even though life-cycle bias might attenuate the estimated effects on 0-10 year olds, these results indicate that the 1980-1982 recession had considerable long-run effects on income, wages, and poverty for individuals who were 0-19 years old in 1979.

Standard models of worker productivity suggest that the recession's negative effects on education should partly explain the negative effects on income and wages. To quantify the importance of this channel, I estimate regressions that control for educational attainment (high school or GED attainment, college attendance, two-year college degree attainment, and four-year college degree attainment). If these controls eliminate the relationship between exposure to the recession and income, then education could be the relevant mechanism. However, this approach might overstate the importance of education by attributing to it any unobserved determinant of income that is positively correlated with education, such as unmeasured cognitive skills. As seen in Table 1.6, the point estimates indicate that education can explain up to 56 percent of the effect on earned income, 90 percent of the effect on wages, and 42 percent of the effect on family income for individuals age 0-10 in 1979. For individuals age 11-19 in 1979, education can explain up to 47 percent of the effect on earned income, 37 percent of the effect on wages, and 32 percent of the effect on family income.

Conditional on worker characteristics, wages vary considerably across local labor markets, partly due to differences in employer characteristics and total factor productivity (Moretti, 2011). As a result, the long-run effects on income and wages might arise partly from individuals' tendency to live and work near their place of birth, which experienced a persistent decrease in earnings per capita following the 1980-1982 recession. To examine this, I estimates regressions that control for individuals' commuting zone of residence. This approach could overstate the importance of location if unobserved determinants of income are positively correlated with living in a high income labor market. For individuals age 0-10 in 1979, the point estimates in Table 1.6 indicate that location can explain up to 65 percent of the effect on earned income, 101 percent of the effect on wages, and 53 percent of the effect on family income. In sum, education and location could explain a substantial, and similar amount, of the long-run effects on income and wages.

Simple calculations reinforce the importance of education in explaining long-run effects on income and wages. The effects of the recession on college degree attainment and the cross-sectional income-schooling gradient suggest that a 10 percent decrease in earnings per capita from 1978-1982 should reduce earned income for individuals age 0-10 in 1979 by 2.0 percent through the education channel alone.[49] This accounts for 63 percent of the estimated effect on personal income in Table 1.5, which is a 3.2 percent decrease. For individuals age 0-10 in 1979, the predicted negative effects on wages and family income are 1.6 and 1.8 percent, while the estimated negative effects are 1.8 and 3.7 percent.

Previous studies find that individuals who graduate from college during a recession experience a lasting decline in earnings and wages relative to individuals who graduate into a stronger economy, partly due to working at lower paying employers (Kahn, 2010; Oreopoulos, von Wachter and Heisz, 2012). This channel could explain some of the decrease in income and wages for individuals age 18-22 in 1979, as suggested by the similarity between previous estimates and the results in Table 1.5.[50] The estimates in Table 1.5 are also within the relatively wide range of effects predicted by studies on the long-run effects of parental job displacement (Page, Stevens and Lindo, 2007; Bratberg, Nilsen and Vaage, 2008; Oreopoulos, Page and Stevens, 2008; Hilger, 2016).[51]

[49] In 2000 Census and 2001-2013 ACS data, the observed Mincerian returns to a two- and four-year degree are 0.285 log points and 0.705 log points for earned income. The returns in wages are 0.237 and 0.623, and the the returns in family income are 0.294 and 0.696. I calculate these coefficients using an OLS regression for individuals born in the U.S. from 1950-1979 who were age 25-64 in the survey year, controlling for a cubic in potential experience and indicators for non-white, male, and survey year.

[50] The estimates in Kahn (2010) suggest that a 10 percent decrease in earnings per capita from 1978-1982 might decrease wages of four-year college graduates by up to 7.6 percent in the long-run. In particular, she finds that a 1 percentage point increase in the state-level unemployment rate is associated with up to a 9.8 percent decrease in wages 15 years after graduating from college (see column 4 of her Table 4); at the county-level, a 10 percent decrease in earnings per capita from 1978-1982 is associated with a 7.7 percentage point increase in the unemployment rate, conditional on state fixed effects. Because 29.2 percent of individuals age 20-28 in 1979 obtain a four-year college degree, Kahn's estimates imply a decrease in wages for 20-28 year olds of 2.2 percent ($= 0.292 \times 0.076$) if only college graduates' wages decline. This prediction is slightly more negative than the point estimate in column 3 of Table 1.5, but within the 95 percent confidence interval. Some estimates in Kahn (2010) and the estimates in Oreopoulos, von Wachter and Heisz (2012) imply that college graduates' wages and earnings recover after 10 or 15 years, broadly consistent with the estimates in Table 1.5.

[51] Previous studies find that parental job displacement is associated with a 0 to 9 percent decrease in earnings (Page, Stevens and Lindo, 2007; Bratberg, Nilsen and Vaage, 2008; Oreopoulos, Page and Stevens, 2008; Hilger, 2016) for children age 10-19 at the time of job loss. If job loss generated all of the county-level decrease in earnings per capita from 1978-1982 and the recession only affected children whose parents lost a job, these estimates suggest that a 10 percent decrease in earnings per capita would decrease earned income by 0-9 percent.

My estimates also relate to recent work by Chetty and Hendren (2016*a*,*b*), who estimate the effects on children of moving to a better neighborhood (i.e., county or commuting zone). Chetty and Hendren study what happens when people move to worse (or better) places, where neighborhood quality is fixed over time and measured by the income of permanent residents, while I study what happens when the 1980-1982 recession makes places worse. For individuals age 0-10 in 1979, a 1 SD increase in recession severity has similar effects on family income as a 0.5 SD decrease in the county quality measure of Chetty and Hendren throughout childhood.[52] Chetty and Hendren (2016*b*) do not find a strong correlation between local economic conditions in a county or CZ and that place's long-run effect on individuals. As they make clear, their empirical strategy does not identify the causal effect of local economic conditions on long-run outcomes. In contrast, my empirical strategy does.

1.7 Conclusion: The Long-Run Effects of Recessions

This paper provides new evidence on the long-run effects of recessions on education and income. Using confidential Census data linked to county of birth and a generalized difference-in-differences framework, I estimate the long-run effects of the 1980-1982 recession on individuals who were children, adolescents, and young adults when the recession began. I find that the recession generated sizable long-run reductions in education and income. For individuals age 0-10 in 1979, a 10 percent decrease in real earnings per capita in individuals' birth county during the recession leads to a 3.0 percentage point (9.4 percent) decrease in four-year college degree attainment, a $1,314 (3.2 percent) decrease in earned income, and a 1.7 percentage point (13.9 percent) increase in the probability of living in poverty as of 2000-2013.

My estimates, combined with the large number of potentially affected individuals, suggest that the 1980-1982 recession could depress aggregate economic output today. To provide some

[52]A 1 SD increase in the severity of the recession amounts to a 11.4 percent decrease in earnings per capita from 1978-1982, which leads to a 4.2 percent ($= 0.114 \times 0.366$) decrease in family income. Chetty and Hendren find that each additional year of childhood spent in a 1 SD worse county leads to a 0.4 percent decrease in family earnings at age 26, so spending 20 years in a 1 SD worse county leads to an 8 percent decrease in earnings (Chetty and Hendren, 2016*b*, p. 14).

evidence on this possibility, Table 1.7 reports simple back of the envelope calculations that scale my difference-in-differences estimates by the 105 million individuals who were born in the U.S. from 1951-1979.[53] If I assume that all counties would have experienced no change in real earnings per capita in the absence of the recession, these calculations suggest that the recession led to 899,000 fewer four-year college graduates, \$64 billion less earned income per year, and 554,000 more adults living in poverty each year. If I instead assume that all counties would have experienced the average level of pre-recession growth in earnings per capita, these calculations suggest that the recession led to 2.1 million fewer four-year college graduates, \$145 billion less earned income per year, and 1.3 million more adults living in poverty each year.[54] These numbers amount to 1.3-3.0 percent of the stock of college-educated adults in 2015, 0.4-0.8 percent of GDP in 2015 and 0.9-2.0 percent of GDP in 1979, and 1.3-2.9 percent of the number of individuals in poverty in 2015.[55] While these simple calculations could understate or overstate the aggregate effects, the 1980-1982 recession might considerably reduce aggregate economic output today.[56]

This paper shows that the 1980-1982 recession persistently decreased earnings per capita in negatively affected counties, and individuals born in these counties have less education and income as adults. While I have not examined whether other recessions have similar long-run effects, Figure 1.7 demonstrates a novel stylized fact that provides reason for concern: every U.S. recession since

[53] In particular, I calculate the aggregate effect of the recession on some long-run outcome for individuals who were age a in 1979 as $\sum_c N_{a,c}(R_c^{78-82} - R_c^{CF})(-\hat{\pi}_a)$, where $N_{a,c}$ is the number of individuals born in county c who would have been age a in 1979, R_c^{78-82} is the observed change in log real earnings per capita from 1978-1982, R_c^{CF} is the counterfactual change in log real earnings per capita, and $\hat{\pi}_a$ is the difference-in-differences estimate from equation (1.3), multiplied by -1 because I now use changes, instead of decreases, in log earnings per capita.

[54] From 1969-1978, real earnings per capita grew by 1.9 percent per year on average.

[55] There were 69 million individuals with a four-year college degree in 2015 (Ryan and Bauman, 2016). In 2014 dollars, U.S. GDP was \$7.2 billion in 1979 and \$18.2 billion in 2015, and there were 43 million individuals living in poverty in 2015 (Proctor, Semega and Kollar, 2016).

[56] These simple calculations do not capture cohort-wide effects or general equilibrium adjustments, and they rely on the linear approximation used in my difference-in-differences model. As a result, they could understate or overstate the true aggregate effects. The resulting bias from not capturing cohort-wide effects is unclear, as these effects could be positive or negative. For example, negative effects could arise from a nationwide increase in parental stress which harmed long-run outcomes; in this case, the back of the envelope calculations would be too conservative, though it is difficult to say by how much. General equilibrium adjustments suggest that back of the envelope calculations might overstate the aggregate effect of the recession. For example, increasing the college degree attainment of individuals born in one county might decrease the attainment of individuals born in other counties due to less than perfect elasticity of supply of college education (Bound and Turner, 2007). Furthermore, these calculations are only for individuals born from 1951-1979, and the recession could have negative effects on individuals born after 1979, including the children of those born from 1951-1979.

1973 has led to a persistent relative decrease in earnings per capita in negatively affected counties.[57] The 1980-1982 recession is not unique in its persistent effects on county-level earnings per capita, which suggests that it might not be unique in its long-run effects on children. Directly examining the long-run effects of other recessions would be valuable.

These results raise several questions for future research. First, can government policies mitigate the long-run effects of recessions? While it is important to account for the costs of any mitigating policy, my results suggest that there could be sizable benefits. Evidence on the mechanisms that underlie recessions' long-run effects could point to potentially effective policies. Second, why do recessions lead to a persistent decline in local economic activity? Evidence on the household- and firm-level behavior that generate this pattern would shed light on the long-run effects of recessions and, more generally, how local labor markets respond to economic shocks.

[57]The figure plots the percent difference in earnings per capita between counties with a more versus less severe recession, normalized to equal zero at the onset of the recession. The counties with a more versus less severe recession are defined separately for each recession.

Table 1.1: Aggregate Employment Changes from 1978-1982, by Industry

	Share of total 1978 employment (1)	Log employment change (2)	Employment change (3)
Panel A: Overall and one-digit industries			
All industries	1.000	0.064	4,545,523
Manufacturing	0.289	-0.045	-880,902
Construction	0.058	-0.043	-170,951
Agriculture, forestry, and fisheries	0.004	0.198	59,091
Transportation and public utilities	0.062	0.070	310,444
Mining	0.012	0.358	353,059
Wholesale trade	0.070	0.082	418,200
Finance, insurance, and real estate	0.070	0.112	576,696
Retail trade	0.206	0.057	840,051
Services	0.221	0.190	3,214,746
Panel B: Two-digit industries with largest employment decrease			
Auto dealers (retail trade)	0.028	-0.120	-212,068
Transportation equipment (manufacturing)	0.025	-0.135	-206,023
Primary metal (manufacturing)	0.017	-0.183	-185,395
Lumber and wood products (manufacturing)	0.011	-0.239	-154,868
General contractors (construction)	0.017	-0.132	-140,851
Textile mill products (manufacturing)	0.013	-0.171	-135,377
Apparel and other textile products (manufacturing)	0.019	-0.098	-120,553
Stone, clay, and glass products (manufacturing)	0.010	-0.157	-92,833
Fabricated metal products (manufacturing)	0.024	-0.059	-91,861
Trucking and warehousing (transportation)	0.019	-0.054	-66,322
Panel C: Two-digit industries with largest employment increase			
Nondurables (wholesale trade)	0.028	0.088	174,462
Social services (services)	0.013	0.183	177,258
Durables (wholesale trade)	0.041	0.074	210,445
Depository institutions (finance)	0.021	0.145	212,866
Oil and gas extraction (mining)	0.005	0.602	284,491
Food stores (retail trade)	0.031	0.141	309,392
Miscellaneous services (services)	0.011	0.376	342,560
Eating and drinking places (retail trade)	0.060	0.118	501,927
Business services (services)	0.038	0.236	678,268
Health services (services)	0.070	0.223	1,166,838

Notes: I construct this table by aggregating county-level data for the continental United States. Because employment is often suppressed at the county-level, I impute employment using the number of establishments and nationwide information on average employment by establishment size, as described in Appendix A.1. Source: Census County Business Patterns

Table 1.2: The Long-Run Effects of the 1980-1982 Recession on Educational Attainment

	Dependent variable:					
	HS/GED attainment (1)	Any college attendance (2)	Any college degree attainment (3)	Four-year college degree attainment (4)	Two-year college degree attainment (5)	Years of schooling (6)
Panel A: Interaction between 1978-1982 decrease in log real earnings per capita and age in 1979						
0-10	0.0394	-0.0377	-0.184***	-0.303***	0.119**	-0.417
	(0.0392)	(0.0522)	(0.0678)	(0.109)	(0.0600)	(0.373)
11-19	0.0380	-0.0987	-0.122**	-0.159**	0.0369	-0.0831
	(0.0311)	(0.0664)	(0.0586)	(0.0801)	(0.0481)	(0.309)
20-28	0.0172	-0.0540	0.0263	0.0306	-0.0043	0.361*
	(0.0263)	(0.0507)	(0.0363)	(0.0426)	(0.0333)	(0.204)
Panel B: Average value of dependent variable in years 2000-2013, by age in 1979						
0-10	0.936	0.588	0.414	0.321	0.093	13.57
11-19	0.932	0.537	0.380	0.288	0.093	13.39
20-28	0.933	0.540	0.381	0.292	0.090	13.41

Notes: Panel A reports estimates of the interaction between the 1978-1982 decrease in log real earnings per capita in individuals' birth county and indicators for age in 1979. The interaction for individuals age 29 is normalized to equal zero. Regressions include fixed effects for race, sex, birth county, age in 1979-by-birth state, and survey year, plus age in 1979 interacted with the 1950-1970 change in log real median family income in individuals' birth county and a cubic in age at time of survey. Regressions are estimated by 2SLS, using the predicted log employment change in all industries from 1978-1982 as an instrumental variable. Standard errors in parentheses are clustered by birth state. The sample in Panel A contains 23.5 million individuals born in the continental U.S. from 1950-1979 with a unique birth county and non-imputed variables. Panel B reports average values of the dependent variable for a comparable sample from publicly available 2000 Census and 2001-2013 ACS data.
Sources: BEA Regional Economic Accounts, Census County Business Patterns, Confidential 2000-2013 Census/ACS data linked to the SSA NUMIDENT file, Publicly available 2000-2013 Census/ACS data from Ruggles et al. (2015)

Table 1.3: The Long-Run Effects of the 1980-1982 Recession on Educational Attainment, Heterogeneity by Sex and Race

	Dependent variable:					
	HS/GED attainment	Any college attendance	Any college degree attainment	Four-year college degree attainment	Two-year college degree attainment	Years of schooling
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Heterogeneity by sex						
Interaction between 1978-1982 decrease in log real earnings per capita and age in 1979 for men						
0-10	-0.0118	-0.148**	-0.213**	-0.261**	0.0476	-0.290
	(0.0495)	(0.0733)	(0.0830)	(0.108)	(0.0469)	(0.422)
11-19	-0.0175	-0.181**	-0.188***	-0.165*	-0.0235	-0.280
	(0.0473)	(0.0878)	(0.0709)	(0.0876)	(0.0443)	(0.397)
20-28	-0.0081	-0.0737	-0.0081	0.0050	-0.0131	0.234
	(0.0392)	(0.0676)	(0.0480)	(0.0621)	(0.0443)	(0.303)
Interaction between 1978-1982 decrease in log real earnings per capita and age in 1979 for women						
0-10	0.0864*	0.0793	-0.146**	-0.335***	0.189**	-0.495
	(0.0469)	(0.0619)	(0.0723)	(0.118)	(0.0873)	(0.416)
11-19	0.0895**	-0.0078	-0.0484	-0.147*	0.0983	0.136
	(0.0394)	(0.0605)	(0.0666)	(0.0847)	(0.0728)	(0.343)
20-28	0.0405	-0.0239	0.0684	0.0609	0.0074	0.512*
	(0.0367)	(0.0499)	(0.0497)	(0.0459)	(0.0346)	(0.287)
p-value, equal effects	0.007	0.001	0.034	0.014	0.170	0.013
Panel B: Heterogeneity by race						
Interaction between 1978-1982 decrease in log real earnings per capita and age in 1979 for whites						
0-10	-0.0045	-0.141**	-0.286***	-0.403***	0.116*	-0.989**
	(0.0334)	(0.0666)	(0.0949)	(0.146)	(0.0632)	(0.483)
11-19	0.0031	-0.166**	-0.192**	-0.229**	0.0372	-0.493
	(0.0278)	(0.0751)	(0.0757)	(0.108)	(0.0523)	(0.371)
20-28	0.0054	-0.0755	0.0081	0.0137	-0.0056	0.271
	(0.0234)	(0.0511)	(0.0409)	(0.0515)	(0.0369)	(0.205)
Interaction between 1978-1982 decrease in log real earnings per capita and age in 1979 for non-whites						
0-10	0.425**	0.468**	0.168	-0.0064	0.174**	2.153*
	(0.181)	(0.199)	(0.144)	(0.110)	(0.0887)	(1.188)
11-19	0.325***	0.324**	0.175	0.111	0.0633	2.032**
	(0.0977)	(0.138)	(0.116)	(0.0926)	(0.0780)	(0.829)
20-28	0.134*	0.142	0.134	0.0956	0.0388	1.075*
	(0.0686)	(0.115)	(0.102)	(0.0912)	(0.0797)	(0.585)
p-value, equal effects	0.006	0.014	0.046	0.056	0.739	0.013

Notes: See notes to Table 1.2. I estimate separate 2SLS regressions for men and women (Panel A) and whites and non-whites (Panel B). The p-value is for the null hypothesis that the effects of the recession are equal for men and women or whites and non-whites.

Sources: BEA Regional Economic Accounts, Census County Business Patterns, Confidential 2000-2013 Census/ACS data linked to the SSA NUMIDENT file

Table 1.4: The Long-Run Effects of the 1980-1982 Recession on Four-Year College Degree Attainment, Heterogeneity by Features of Birth State and County

Type of heterogeneity:	State recession		State mean transfers		State transfer slope		Poverty in county	
	More severe (1)	Less severe (2)	Less generous (3)	More generous (4)	Less progressive (5)	More progressive (6)	Less poverty (7)	More poverty (8)
Interaction between 1978-1982 decrease in log real earnings per capita and age in 1979								
0-10	-0.179**	-0.387**	-0.378**	-0.257*	-0.339**	-0.235*	-0.252	-0.337
	(0.0719)	(0.192)	(0.173)	(0.133)	(0.149)	(0.139)	(0.905)	(0.521)
11-19	-0.0940	-0.205	-0.161	-0.157	-0.193**	-0.0955	-0.119	-0.185
	(0.0631)	(0.138)	(0.131)	(0.103)	(0.0965)	(0.152)	(0.272)	(0.193)
20-28	0.0190	0.0365	0.0130	0.0417	0.0135	0.0625	0.0449	0.0212
	(0.0701)	(0.0586)	(0.0657)	(0.0586)	(0.0509)	(0.0899)	(0.196)	(0.125)
p-value, equal effects	0.616		0.498		0.956		0.998	

Notes: See notes to Table 1.2. I estimate separate 2SLS regressions for each dimension of heterogeneity. The p-value is for the null hypothesis that the effects of the recession are equal across columns. States with a more severe recession are those with an above-median decrease in log real earnings per capita from 1978-1982. States with less generous mean transfers are those with below-median transfers per capita in 1970, conditional on demographic and economic covariates. States with a less progressive transfer slope are those with an above-median slope coefficient from a regression of log transfers per capita on log median family income in 1970, conditional on demographic and economic covariates. Counties with less poverty are those with a below median poverty rate in 1970. See text for details.

Sources: BEA Regional Economic Accounts, Census County Business Patterns, Confidential 2000-2013 Census/ACS data linked to the SSA NUMIDENT file, Census County Data Book

Table 1.5: The Long-Run Effects of the 1980-1982 Recession on Income, Wages, and Poverty

	Dependent variable:				
	Log personal income (1)	Log earned income (2)	Log hourly wage (3)	Log family income (4)	In poverty (5)
Panel A: Interaction between 1978-1982 decrease in log real earnings per capita and age in 1979					
0-10	-0.217*	-0.321**	-0.178*	-0.366**	0.168***
	(0.120)	(0.126)	(0.0994)	(0.166)	(0.0493)
11-19	-0.228**	-0.272***	-0.318***	-0.351***	0.0766**
	(0.0976)	(0.0984)	(0.115)	(0.122)	(0.0349)
20-28	-0.0872	-0.0819	-0.118*	-0.135*	0.0182
	(0.0817)	(0.0902)	(0.0699)	(0.0760)	(0.0254)
Panel B: Average value of dependent variable in years 2000-2013, by age in 1979, in levels					
0-10	42,666	40,942	25.52	80,892	0.122
11-19	51,232	48,391	29.81	93,896	0.103
20-28	54,089	48,880	32.04	98,157	0.092

Notes: See notes to Table 1.2. The sample in columns 1-4 contains 15.6 million individuals born from 1950-1979 in the continental U.S. with a unique birth county, non-imputed variables, and positive values of family income, earned income, personal income, and wage. The sample in column 5 contains 18.4 million individuals born from 1950-1979 in the continental U.S. with a unique birth county and non-imputed variables. All monetary variables are in 2014 dollars.

Sources: BEA Regional Economic Accounts, Census County Business Patterns, Confidential 2000-2013 Census/ACS data linked to the SSA NUMIDENT file, Publicly available 2000-2013 Census/ACS data from Ruggles et al. (2015)

Table 1.6: The Long-Run Effects of the 1980-1982 Recession on Income and Wages, Conditional on Educational Attainment and Commuting Zone of Residence

	Interaction between 1978-1982 decrease in log real earnings per capita and age in 1979			Share of effect explained by	
				Education	CZ of Residence
	(1)	(2)	(3)	(4)	(5)
Panel A: Dependent variable is log earned income					
0-10	-0.321**	-0.143	-0.114	0.555	0.645
	(0.126)	(0.108)	(0.110)		
11-19	-0.272***	-0.143	-0.121	0.474	0.555
	(0.0984)	(0.0878)	(0.0798)		
20-28	-0.0819	-0.0601	-0.0165	0.266	0.799
	(0.0902)	(0.0883)	(0.0963)		
Panel B: Dependent variable is log hourly wage					
0-10	-0.178*	-0.0171	0.0025	0.904	1.014
	(0.0994)	(0.0854)	(0.0769)		
11-19	-0.318***	-0.202**	-0.185**	0.365	0.418
	(0.115)	(0.0912)	(0.0820)		
20-28	-0.118*	-0.0971	-0.0620	0.177	0.475
	(0.0699)	(0.0638)	(0.0624)		
Panel C: Dependent variable is log family income					
0-10	-0.366**	-0.214	-0.174	0.415	0.525
	(0.166)	(0.145)	(0.130)		
11-19	-0.351***	-0.240**	-0.209**	0.316	0.405
	(0.122)	(0.101)	(0.0903)		
20-28	-0.135*	-0.116*	-0.0752	0.141	0.443
	(0.0760)	(0.0687)	(0.0757)		
Conditional on					
Education		X			
CZ of residence			X		

Notes: See notes to Table 1.2. Education controls include high school or GED attainment, college attendance, two-year college degree attainment, and four-year college degree attainment. CZ of residence control is a fixed effect. Column 4 equals the ratio of column 1 minus column 2 and column 1. Column 5 equals the ratio of column 1 minus column 3 and column 1. The sample contains 15.6 million individuals born from 1950-1979 with a unique birth county, non-imputed variables, and positive values of family income, earned income, personal income, and wage.

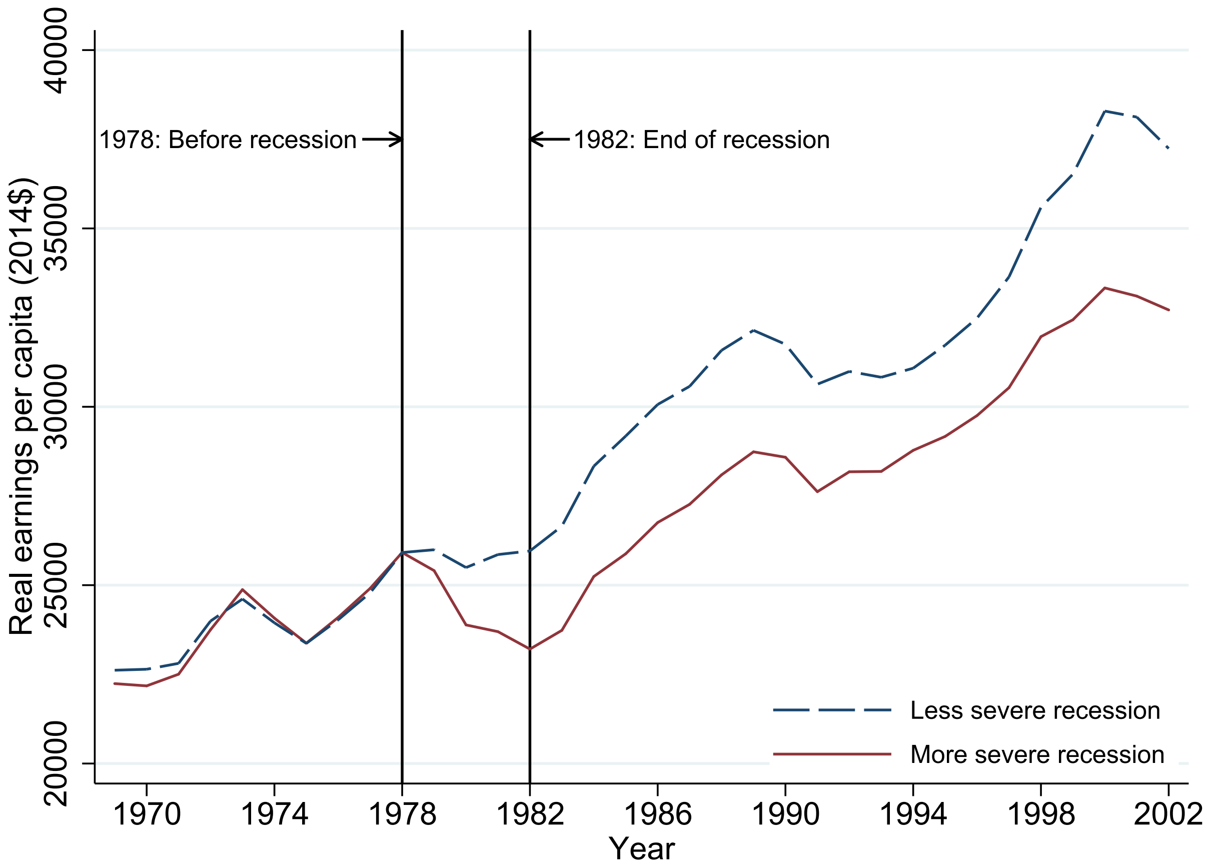
Sources: BEA Regional Economic Accounts, Census County Business Patterns, Confidential 2000-2013 Census/ACS data linked to the SSA NUMIDENT file

Table 1.7: Back of the Envelope Calculations of the Aggregate Long-Run Effects of the 1980-1982 Recession

		Counterfactual 1: No real earnings per capita growth, 1978-1982			Counterfactual 2: Trend real earnings per capita growth, 1978-1982		
	Number of births, mil. (1)	Four-year college graduates (2)	Earned income, bil. $ (3)	Adults living in poverty (4)	Four-year college graduates (5)	Earned income, bil. $ (6)	Adults living in poverty (7)
Age in 1979							
0-10	36.0	-643,700	-27.9	356,903	-1,473,351	-63.9	816,908
11-19	34.1	-324,478	-26.9	156,321	-737,019	-61.0	355,067
20-28	34.6	68,998	-9.0	41,038	149,447	-19.6	88,887
0-28	104.8	-899,180	-63.8	554,261	-2,060,924	-144.5	1,260,861

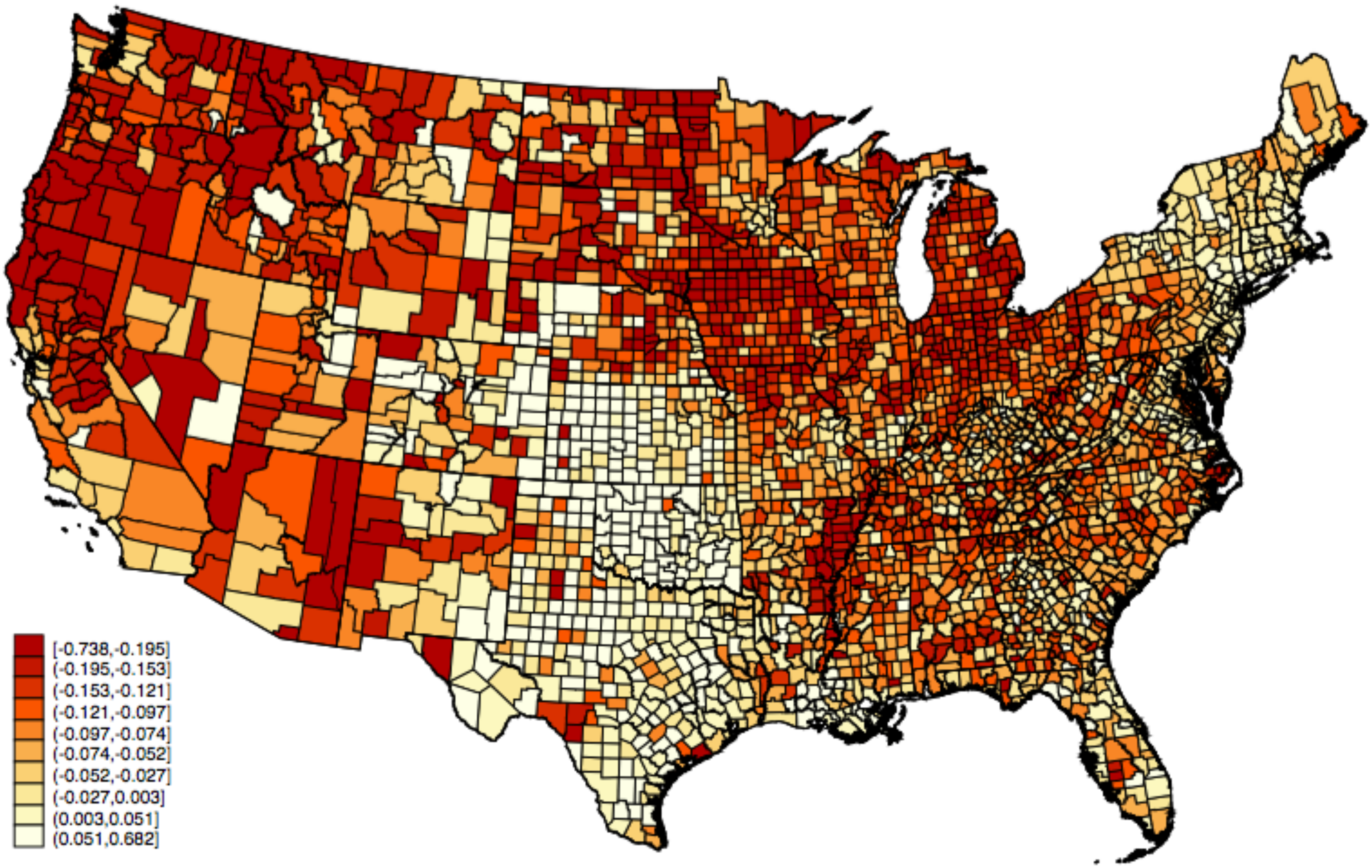
Notes: Table displays back of the envelope calculations of the aggregate long-run effects of the 1980-1982 recession. For individuals who were a years old in 1979, I calculate these as $\sum_c N_{a,c}(R_c^{78-82} - R_c^{CF})(-\hat{\pi}_a)$, where $N_{a,c}$ is the number of individuals born in county c net of infant mortality, R_c^{78-82} is the observed change in log real earnings per capita from 1978-1982 in county c, R_c^{CF} is the counterfactual change in log real earnings per capita from 1978-1982, and $\hat{\pi}_a$ is the difference-in-differences estimate. In counterfactual 1, I set $R_c^{CF} = 0$ and in counterfactual 2, $R_c^{CF} = 0.076$, which corresponds to the average annual growth in earnings per capita from 1969-1978 of 1.9 percent. Column 1 reports the total number of births for each age group, net of infant mortality ($\sum_c N_{a,c}$). Columns 2 and 5 use difference-in-differences estimates from column 4 of Table 1.2. Columns 3 and 6 use estimates from column 2 of Table 1.5. Columns 4 and 7 use estimates from column 5 of Table 1.5.
Sources: BEA Regional Economic Accounts, Census County Business Patterns, Confidential 2000-2013 Census/ACS data linked to the SSA NUMIDENT file, Birth and infant mortality data from Bailey et al. (2016)

Figure 1.1: Normalized Mean Real Earnings per Capita, by County-Level Severity of the 1980-1982 Recession



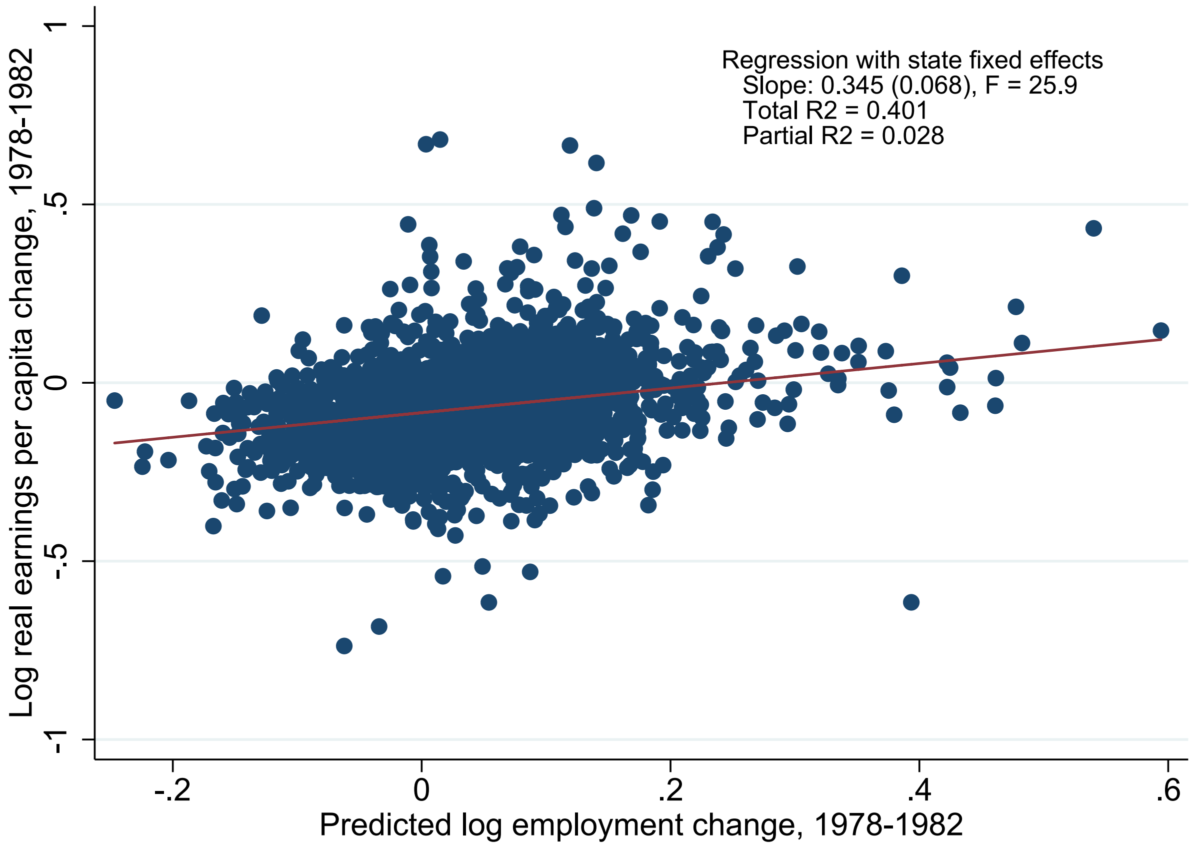
Notes: Figure displays population-weighted mean real earnings per capita, among counties with a below and above median 1978-1982 decrease in log real earnings per capita. I calculate the median using 1978 population weights. I adjust the less severe recession line to equal the more severe recession line in 1978, which amounts to a downward shift of $2,110. Sample contains 3,076 counties in the continental U.S.
Source: BEA Regional Economic Accounts

Figure 1.2: Log Real Earnings per Capita Change, 1978-1982



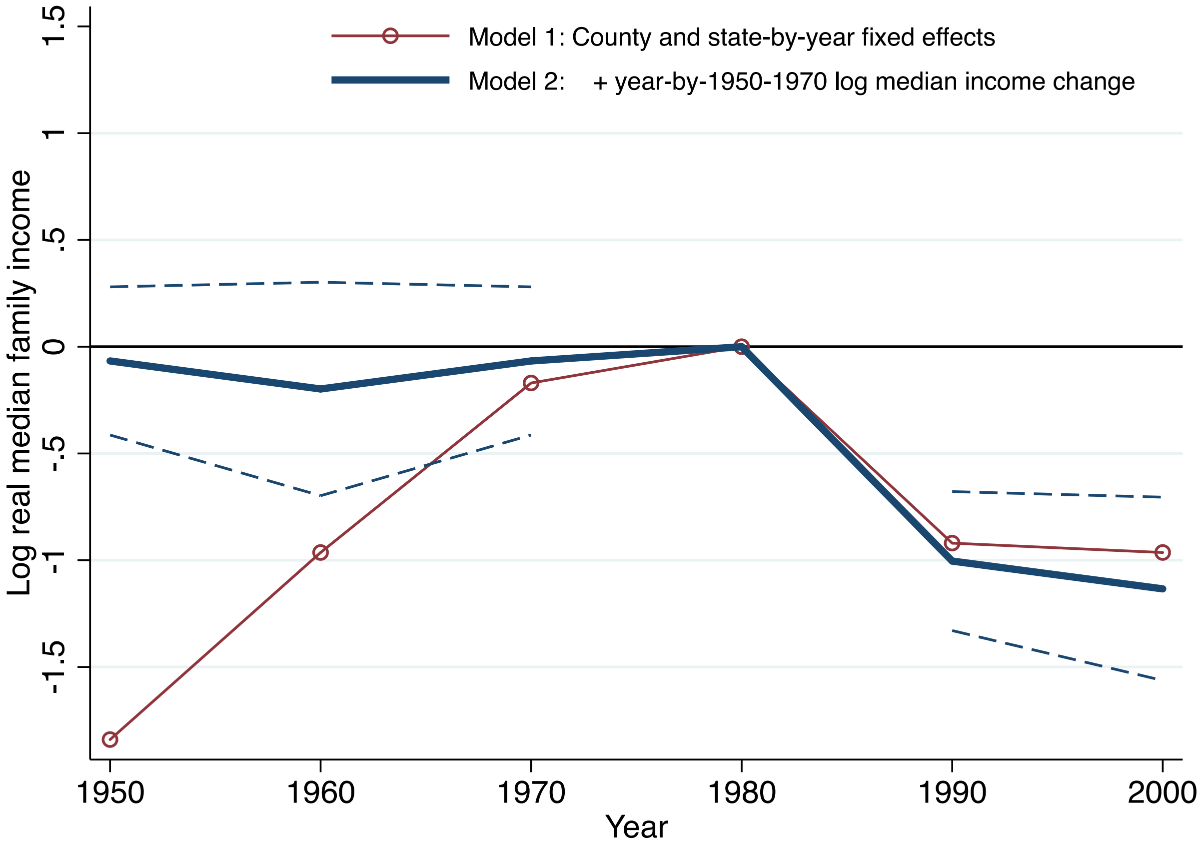
Notes: Figure displays the county-level change in log real earnings per capita from 1978-1982, which I use to measure the severity of the 1980-1982 recession. Categories correspond to unweighted deciles, with darker shades of red representing a more severe recession.
Source: BEA Regional Economic Accounts

Figure 1.3: Log Real Earnings per Capita Change and Predicted Log Employment Change, 1978-1982



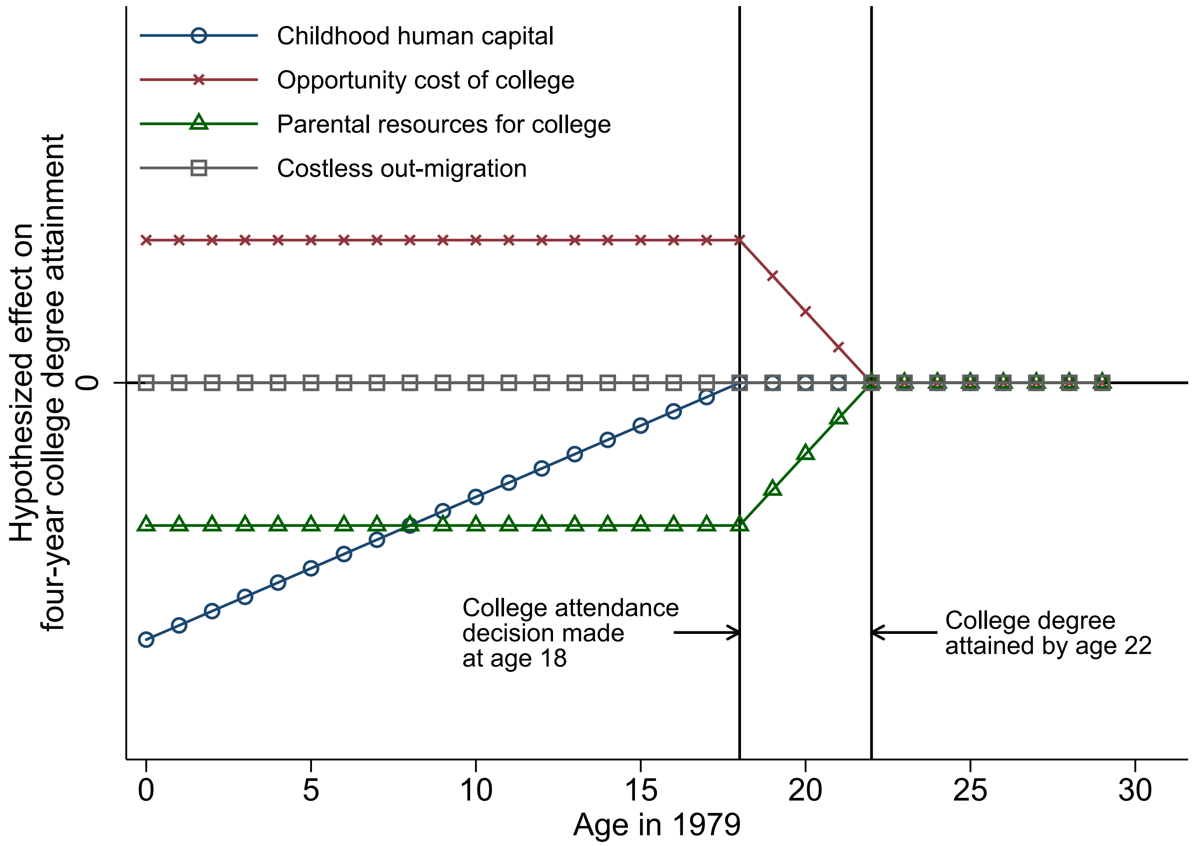
Notes: Predicted log employment change is constructed using a county's 1976 industrial structure and the industry-level log employment change from 1978-1982 in other states within the same region, as defined in equation (1.1). The reported estimates and best fit line come from a regression that includes state fixed effects and clusters standard errors by state. Sample contains 3,076 counties in the continental U.S.
Sources: BEA Regional Economic Accounts and Census County Business Patterns

Figure 1.4: Log Real Median Family Income Before and After the 1980-1982 Recession, 2SLS Estimates



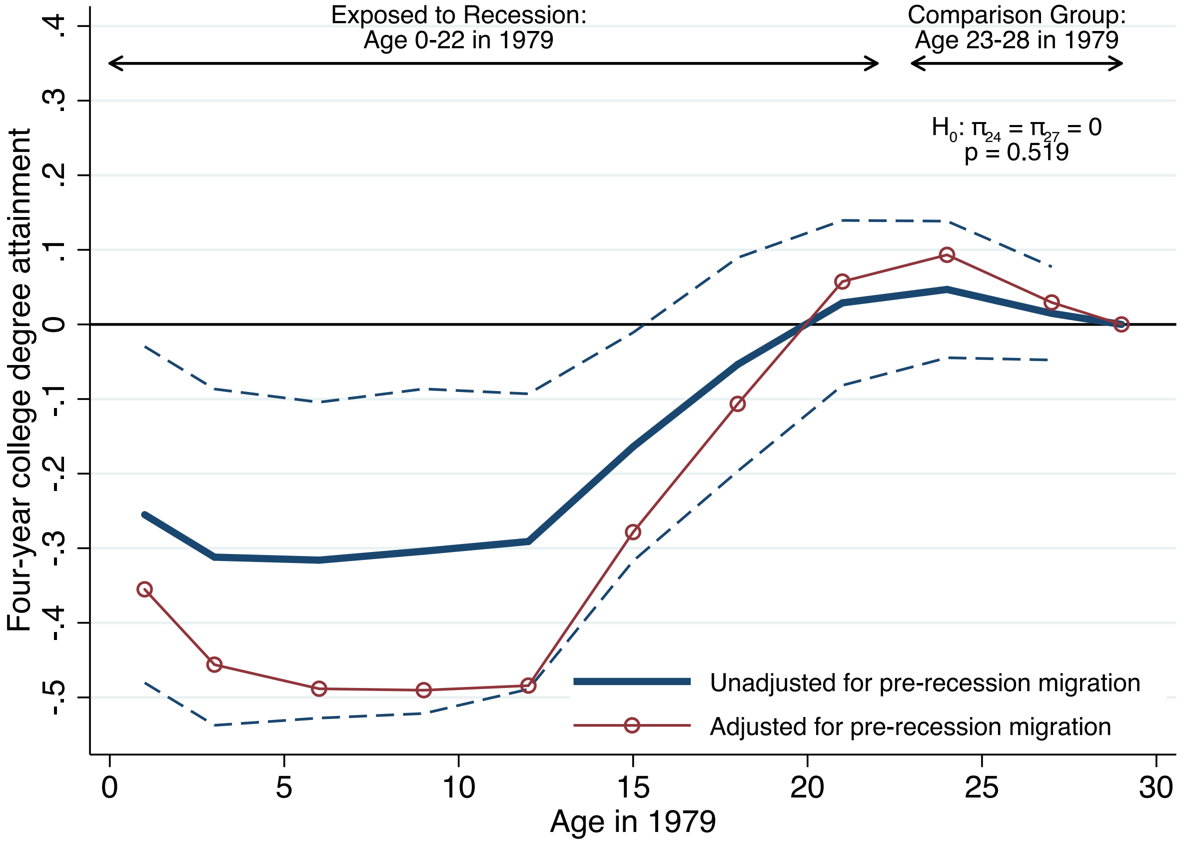
Notes: Figure plots the estimated coefficients on interactions between year and the 1978-1982 decrease in log real earnings per capita, where the coefficient for 1980 is normalized to equal zero. The dependent variable is log real median family income for 1950-1990 and log real median household income for 2000. Regressions are estimated by 2SLS, using the predicted log employment change from 1978-1982 as an instrumental variable. The dashed lines are pointwise 95 percent confidence intervals based on standard errors clustered by state. Sample is limited to the 2,550 counties with less than 5 percent of 1976 employment in the mining sector.
Sources: BEA Regional Economic Accounts, Census County Business Patterns, Census County Data Books, Minnesota Population Center (2011)

Figure 1.5: Hypothesized Long-Run Effects of the 1980-1982 Recession on College Degree Attainment, by Underlying Channel



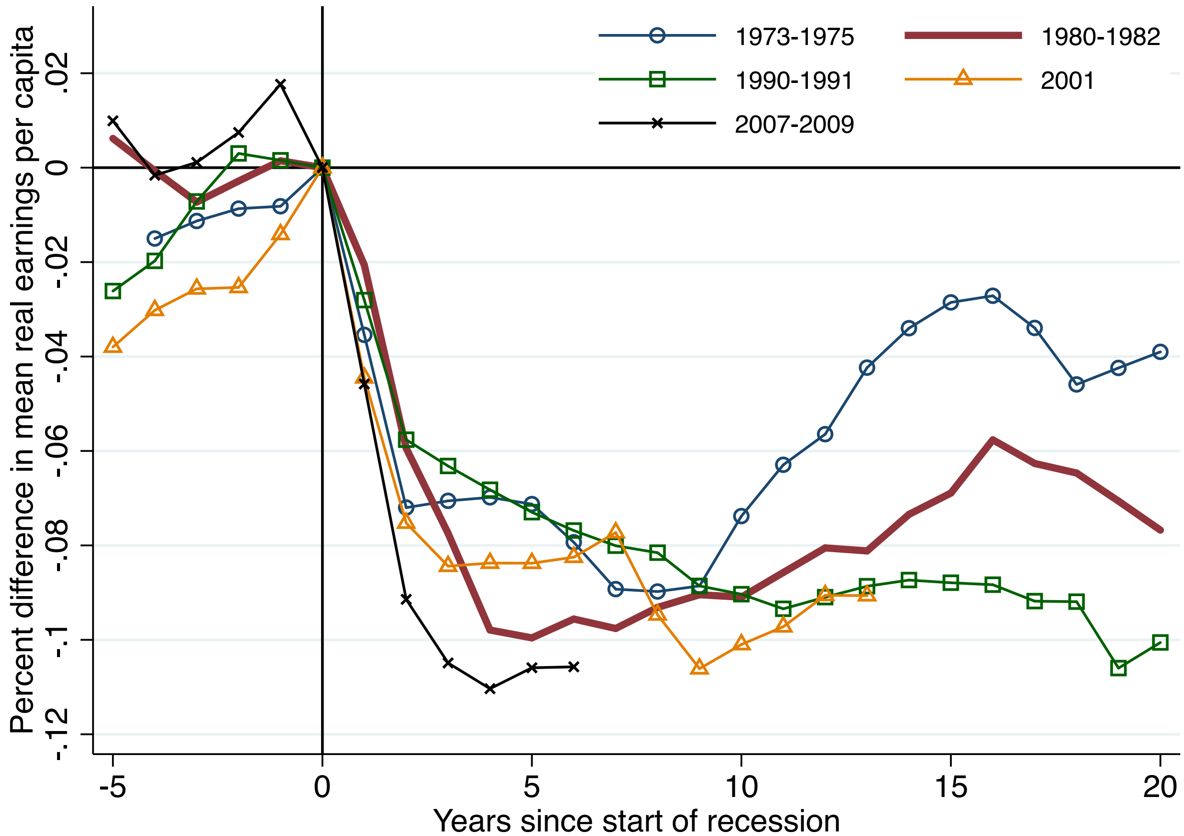
Notes: Figure displays hypothesized effects of the 1980-1982 recession on college degree attainment from different underlying channels. I model the recession as a persistent, one-time decrease in local labor demand, which is consistent with the effects of the 1980-1982 recession on counties. The recession could decrease college degree attainment by reducing parental and community investments in childhood human capital. The recession could increase college degree attainment by reducing the opportunity cost of forgone earnings. In the presence of credit constraints, the recession could decrease college degree attainment by reducing parental resources to finance college. There might be no effect if parents out-migrate from negatively affected areas and there are no disruption costs of moving. See text for additional discussion.

Figure 1.6: The Long-Run Effects of the 1980-1982 Recession on Four-Year College Degree Attainment



Notes: Figure plots estimates of the interaction between the 1978-1982 decrease in log real earnings per capita in individuals' county of birth and indicators for age in 1979. The interaction for individuals age 29 is normalized to equal zero. The dependent variable is an indicator for four-year college degree attainment. The regression includes fixed effects for race, sex, birth county, age in 1979-by-birth state, and survey year, plus age in 1979 interacted with the 1950-1970 change in log real median family income in individuals' birth county and a cubic in age at time of survey. The regression is estimated by 2SLS, using the predicted log employment change from 1978-1982 as an instrumental variable. The dashed lines are pointwise 95 percent confidence intervals based on standard errors clustered by state. To increase precision, I combine ages 0-1, 2-4, 5-7, 8-10, 11-13, 14-16, 17-19, 20-22, 23-25, and 26-28. The sample contains 23.5 million individuals born in the continental U.S. from 1950-1979 with a unique birth county and non-imputed variables. The line that adjusts for pre-recession migration divides the unadjusted estimates by the coefficient from regressing the 1978-1982 decrease in log real earnings per capita in county of residence on the 1978-1982 decrease in log real earnings per capita in county of birth, using individuals born from 1990-2013.
Sources: BEA Regional Economic Accounts, Census County Business Patterns, Confidential 2000-2013 Census/ACS data linked to the SSA NUMIDENT file

Figure 1.7: Percent Difference in Mean Real Earnings per Capita between Counties with More versus Less Severe Recession



Notes: Figure displays the difference in population-weighted mean real earnings per capita between counties with a more versus less severe recession, as a share of the less severe recession mean. Separately for each recession, I classify counties with a more recession as those with an above median decrease in log real earnings per capita from 1973-1975, 1978-1982, 1989-1991, 2000-2002, and 2007-2010. I calculate the medians using population weights in the starting years. The starting years are the years in which aggregate real earnings per capita decline. Each line is normalized to equal zero in the starting year via a parallel shift upwards or downwards. For reference, NBER recession dates are November 1973 to March 1975; January 1980 to July 1980 and July 1981 to November 1982; July 1990 to March 1991; March 2001 to November 2001; and December 2007 to June 2009. Sample contains 3,076 counties in the continental U.S.
Source: BEA Regional Economic Accounts

CHAPTER II

Social Interactions and Location Decisions: Evidence from U.S. Mass Migration

2.1 Introduction

A large and growing literature finds that social interactions influence many economic outcomes, such as educational attainment, crime, and employment (for recent reviews, see Blume et al., 2010; Epple and Romano, 2011; Munshi, 2011; Topa, 2011). While economists have long-recognized the role of location decisions in shaping individual and aggregate economic outcomes, there is little evidence on the importance of social interactions in location decisions, and even less evidence on the types of individuals or economic environments for whom social interactions are most important. Evidence on the magnitude and nature of social interactions in location decisions informs theoretical models of migration, the role of migration in equilibrating local labor markets, and the likely impacts of policies that affect migration incentives.

This paper provides new evidence on the magnitude and nature of social interactions in location decisions. We focus on the mass migrations of African Americans from the South and whites from the Great Plains in the mid-twentieth century. The millions of moves made during these episodes yield particularly valuable settings for studying the long-run effects of social interactions on location decisions. We use confidential administrative data that measure town of birth and county of residence at old age for most of the U.S. population born between 1916 and 1936.

Detailed geographic information allows us to separate birth town-level social interactions from other determinants of location decisions, such as expected wages or moving costs. For example, we observe that 51 percent of African-American migrants born from 1916-1936 in Pigeon Creek, Alabama moved to Niagara County, New York, while less than six percent of black migrants from nearby towns moved to the same county.

To study this context, we develop a new method of characterizing social interactions in location decisions. We formulate an intuitive "social interactions (SI) index," that can be applied to other discrete choice settings. This index allows us to estimate the strength of social interactions for each receiving and sending location, which we can then relate to locations' economic characteristics. Existing methods are not suited to identifying the strength of social interactions for multiple receiving and sending locations. In particular, extending the widely used approach of Bayer, Ross and Topa (2008), who focus on a binomial outcome, to our multinomial-outcome setting could ascribe strong social interactions to popular destinations even if social interactions were relatively unimportant. Under straightforward and partly testable assumptions, our method identifies the effect of social interactions, and the SI index maps directly to social interaction models.

We find very strong social interactions among Southern black migrants and smaller interactions among whites from the Great Plains. Our estimates imply that if we observed one randomly chosen African American move from a birth town to some destination, then on average 1.9 additional black migrants from that birth town would make the same move. For white migrants from the Great Plains, the average is only 0.4, and results for Southern whites are similarly small. Interpreted through the social interactions model of Glaeser, Sacerdote and Scheinkman (1996), our estimates imply that 49 percent of African-American migrants chose their long-run destination because of social interactions, while 16 percent of Great Plains whites were similarly influenced.

To understand the nature of social interactions in location decisions, we examine whether economic characteristics of receiving and sending locations are associated with stronger social interactions. Social interactions among African Americans were stronger in destination counties with a higher share of 1910 employment in manufacturing, a particularly attractive sector for black workers. This evidence highlights an important role for job referrals in determining location decisions, and suggests that job referrals were more valuable in locations with better employment opportunities. We also find that social interactions were weaker in more distant destinations, pointing to the importance of access to information and low mobility costs. Social interactions were stronger in destinations with fewer African Americans in 1900, suggesting that networks helped migrants find opportunities in new places. Social interactions also were stronger in poorer sending counties, consistent with poorer migrants relying more heavily on social networks.

Several pieces of evidence support the validity of our empirical strategy. Our research design asks whether individuals born in the same town were more likely to live in the same destination in old age than individuals born in nearby towns. This design implies that social interaction estimates should not change when controlling for observed birth town level covariates, because geographic proximity controls for the relevant determinants of location decisions. Reassuringly, we find that our estimates are essentially unchanged when adding meaningful covariates. We also estimate strong social interactions in a small number of locations, like Rock County, Wisconsin, for which rich qualitative work supports our findings (Bell, 1933; Rubin, 1960; Wilkerson, 2010).

We believe this paper makes three contributions. First, we develop a new method of characterizing the magnitude and nature of social interactions. Our approach builds on previous work on social interactions (Glaeser, Sacerdote and Scheinkman, 1996; Bayer, Ross and Topa, 2008; Graham, 2008) and can be used to study social interactions in a variety of other settings. Second, we provide new evidence on the importance of social interactions for location decisions and the types of individuals and economic environments for which social interactions are most important. Previous work shows that individuals tend to migrate to the same areas, often broadly defined, as other individuals from the same town or country, but does not isolate the role of social interactions (Bartel, 1989; Bauer, Epstein and Gang, 2005; Beine, Docquier and Ozden, 2011; Giuletti, Wahba and Zenou, 2014; Spitzer, 2014).[1] Third, our results inform landmark migration episodes that have drawn interest from economists for almost a century (Scroggs, 1917; Smith and Welch,

[1] A notable exception is Chen, Jin and Yue (2010), who study the impact of peer migration on temporary location decisions in China, but lack detailed geographic information on where individuals move.

1989; Carrington, Detragiache and Vishwanath, 1996; Collins, 1997; Boustan, 2009, 2010; Hornbeck, 2012; Hornbeck and Naidu, 2014; Johnson and Taylor, 2014; Black et al., 2015*a*; Collins and Wanamaker, 2015). Our empirical evidence complements the small number of possibly unrepresentative historical accounts suggesting that social interactions might have been important in these migration episodes (Rubin, 1960; Gottlieb, 1987; Gregory, 1989).

Our paper also complements interesting work by Chay and Munshi (2015). They find that, above a threshold, migrants born in counties with higher plantation crop intensity tend to move to fewer locations, as measured by a Herfindahl-Hirschman Index, and this non-linear relationship is consistent with a network formation model with fixed costs of participation. We differ from Chay and Munshi (2015) in our empirical methodology, study of white migrants from the Great Plains and South, and examination of how social interactions vary with destination characteristics.

2.2 Historical Background on Mass Migration Episodes

The Great Migration saw nearly six million African Americans leave the South from 1910 to 1970 (Census, 1979). Although migration was concentrated in certain destinations, like Chicago, Detroit, and New York, other cities also experienced dramatic changes. For example, Chicago's black population share increased from two to 32 percent from 1910-1970, while Racine, Wisconsin experienced an increase from 0.3 to 10.5 percent (Gibson and Jung, 2005). Migration out of the South increased from 1910-1930, slowed during the Great Depression, and then resumed forcefully from 1940 to the 1970's. Panel A of Figure 2.1 shows that the vast majority of African American migrants born from 1916-1936, who comprise our analysis sample described below, moved out of the South between 1940 and 1960. Most migrants in these cohorts moved North between age 15 and 35 (Panel A of Appendix Figure B.1).

Several factors contributed to the exodus of African Americans from the South. World War I, which simultaneously led to an increase in labor demand among Northern manufacturers and a decrease in European immigrant labor supply, helped spark the Great Migration, although many underlying causes existed long before the war (Scroggs, 1917; Scott, 1920; Gottlieb, 1987; Marks, 1989; Jackson, 1991; Collins, 1997; Gregory, 2005). The underlying causes included a less developed Southern economy, the decline in agricultural labor demand due to the boll weevil's destruction of crops (Scott, 1920; Marks, 1989, 1991; Lange, Olmstead and Rhode, 2009), widespread labor market discrimination (Marks, 1991), and racial violence and unequal treatment under Jim Crow laws (Tolnay and Beck, 1991).

Migrants tended to follow paths established by railroad lines: Mississippi-born migrants predominantly moved to Illinois and other Midwestern states, and South Carolina-born migrants predominantly moved to New York and Pennsylvania (Scott, 1920; Carrington, Detragiache and Vishwanath, 1996; Collins, 1997; Boustan, 2010; Black et al., 2015*a*). Labor agents, who offered paid transportation, employment, and housing, directed some of the earliest migrants, but their role diminished sharply after the 1920's (Gottlieb, 1987; Grossman, 1989). Most individuals paid for the relatively expensive train fares themselves. In 1918, train fare from New Orleans to Chicago cost $22 per person, at a time when Southern farmers' daily wages typically were less than $1, and wages at Southern factories were less than $2.50 (Henri, 1975). African-American newspapers from the largest destinations circulated throughout the South, providing information on life in the North (Gottlieb, 1987; Grossman, 1989).[2] Blacks attempting to leave the South sometimes faced violence (Scott, 1920; Henri, 1975).

A small number of historical accounts suggest a role for social interactions in location decisions. Social networks, consisting primarily of family, friends, and church members, provided valuable job references or shelter (Rubin, 1960; Gottlieb, 1987). For example, Rubin (1960) finds that migrants from Houston, Mississippi had close friends or family at two-thirds of all initial destinations.[3] These accounts motivate our focus on birth town-level social interactions.

The experience of John McCord, born in Pontotoc, Mississippi, captures many important features of early black migrants' location decision.[4] In search of higher wages, nineteen-year-old

[2]The *Chicago Defender*, perhaps the most prominent African-American newspaper of the time, was read in 1,542 Southern towns and cities in 1919 (Grossman, 1989).

[3]Rubin (1960) studied individuals from Houston, Mississippi because so many migrants from Houston moved to Beloit, Wisconsin; this is clearly not a representative sample.

[4]The following paragraph draws on Bell (1933). See also Knowles (2010).

McCord traveled in 1912 to Savannah, Illinois, where a fellow Pontotoc-native connected him with a job. McCord moved to Beloit, Wisconsin in 1914 after hearing of opportunities there and started within a week as a janitor at the manufacturer Fairbanks Morse and Company. After two years in Beloit, McCord spoke to his manager about returning home for a vacation. The manager asked McCord to recruit workers during the trip. McCord returned with 18 unmarried men, all of whom soon were hired. Thus began a persistent flow of African Americans from Pontotoc to Beloit: among individuals born from 1916-1936, 14 percent of migrants from Pontotoc lived in Beloit's county at old age (see Table 2.2, discussed below).

Migration out of the Great Plains has received less attention from researchers than the Great Migration, but nonetheless represents a landmark reshuffling of the U.S. population. Considerable out-migration from the Great Plains started around 1930 (Johnson and Rathge, 2006). Among whites born in the Great Plains from 1916-1936, the most rapid out-migration occurred from 1940-1960, as seen in Panel B of Figure 2.1. Most migrants in these cohorts left the Great Plains by age 35 (Panel B of Appendix Figure B.1). Explanations for the out-migration include the decline in agricultural prices due to the Great Depression, a drop in agricultural productivity due to drought, and the mechanization of agriculture (Gregory, 1989; Curtis White, 2008; Hurt, 2011; Hornbeck, 2012). Some historical work points to an important role for social interactions in location decisions (Jamieson, 1942; Gregory, 1989).[5]

The mass migrations out of the South and Great Plains are similar on several dimensions. Both episodes featured millions of long-distance moves, as individuals sought better economic and social opportunities. Furthermore, both episodes saw a similar share of the population undertake long-distance moves. Figure 2.2 shows that 97 percent of blacks born in the South and 90 percent of whites born in the Great Plains lived in their birth region in 1910, and out-migration reduced this share to 75 percent for both groups by 1970. Both African American and white migrants experienced discrimination in many destinations, although African Americans faced more severe discrimination and had less wealth (Collins and Margo, 2001; Gregory, 2005). This context in-

[5]Jamieson (1942) finds that almost half of migrants to Marysville, California had friends or family living there.

forms the interpretation of our results on the relationship between social interactions and location decisions.

2.3 Estimating Social Interactions in Location Decisions

We seek to answer two key questions. First, how important were social interactions in the location decisions of migrants from the South and Great Plains? Second, was the strength of social interactions for receiving and sending locations systematically related to locations' economic characteristics? This section describes a new method of characterizing social interactions that can answer these questions.

2.3.1 Data on Location Decisions

We use confidential administrative data to measure location decisions made during the mass migration episodes. In particular, we use the Duke University SSA/Medicare dataset, which covers over 70 million individuals who received Medicare Part B from 1976-2001. The data contain sex, race, date of birth, date of death (if deceased), and the ZIP code of residence at old age (death or 2001, whichever is earlier). In addition, the data include a 12-character string with self-reported birth town information, which is matched to place data, as described in Black et al. (2015*a*). We use the data to measure long-run migration from birth town to destination county for individuals born from 1916-1936; this sample is at the center of both mass migration episodes and likely contains very few parent-child pairs.[6] To improve the reliability of our estimates, we restrict the sample to birth towns with at least ten migrants and group together all destination counties with less than ten migrants from a given birth state.

Panels A and B of Figure 2.3 display the states we include in the South and Great Plains. For migration out of the South, we study individuals born in Alabama, Georgia, Florida, Louisiana, Mississippi, North Carolina, and South Carolina.[7] We define a migrant as someone who moved

[6]Our sample begins with the 1916 cohort because coverage rates are low for prior years (Black et al., 2015*a*) and ends with 1936 because that is the last cohort available in the data.

[7]Alabama, Georgia, Louisiana, Mississippi, and the Carolinas shared an economic and demographic structure that

out of the 11 Confederate states.[8] For migration out of the Great Plains, we study individuals born in Kansas, Oklahoma, Nebraska, North Dakota, and South Dakota. We define a migrant as someone who moved out of the Great Plains and a border region, shaded in light grey in Panel B.[9] We make these choices to focus on the long-distance moves that characterize both migration episodes.

Our data capture long-run location decisions, as we only observe an individual's location at birth and old age. We cannot identify return migration: if an individual moved from Mississippi to Wisconsin, then returned to Mississippi at age 60, we do not count that person as a migrant. We also do not observe individuals who die before age 65 or do not enroll in Medicare. We discuss the implications of these measurement issues below.

2.3.2 Econometric Model: The Social Interactions Index

We first introduce some notation and discuss the basic idea underlying our approach to estimating social interactions.[10] Let $D_{i,j,k} = 1$ if migrant i moves from birth town j to destination county k and $D_{i,j,k} = 0$ if migrant i moves elsewhere. The probability of a migrant born in town j choosing destination k is $P_{j,k} \equiv \mathbb{E}[D_{i,j,k}]$. This probability reflects individuals' preferences, resources, and the expected return to migration, but does not depend on other individuals' realized location decisions. The number of people who move from birth town j to destination k is $N_{j,k} \equiv \sum_{i \in j} D_{i,j,k}$, and the number of migrants from birth town j is $N_j \equiv \sum_k N_{j,k}$.

A key result in the literature is that positive social interactions yield more variance in decisions than would occur in the absence of social interactions (e.g., Glaeser, Sacerdote and Scheinkman, 1996; Bayer, Ross and Topa, 2008; Graham, 2008). To see this, imagine that we observed multiple realizations of $N_{j,k}$ from a fixed data generating process. The variance of location decisions for a

differed from the rest of the South. We include Florida for completeness, though it differed from the other Southern states (Gregory, 2005).

[8]These include the seven states already listed, plus Arkansas, Tennessee, Texas, and Virginia.

[9]This border region includes Arkansas, Colorado, Iowa, Minnesota, Missouri, Montana, New Mexico, Texas, and Wyoming.

[10]Brock and Durlauf (2001) and Blume et al. (2010) provide comprehensive discussions of various approaches to estimating social interaction.

single birth town-destination pair is

$$\mathbb{V}[N_{j,k}] = \sum_{i \in j} \mathbb{V}[D_{i,j,k}] + \sum_{i \neq i' \in j} \mathbb{C}[D_{i,j,k}, D_{i',j,k}]$$
$$= N_j P_{j,k}(1 - P_{j,k}) + N_j(N_j - 1)C_{j,k}, \tag{2.1}$$

where $C_{j,k} \equiv \sum_{i \neq i' \in j} \mathbb{C}[D_{i,j,k}, D_{i',j,k}]/(N_j(N_j - 1))$ is the average covariance of location decisions for two migrants from the same town. Positive social interaction ($C_{j,k} > 0$) clearly increases the variance of location decisions. In a counterfactual world where we observe multiple observations of $N_{j,k}$, we could directly estimate $P_{j,k}$, $\mathbb{V}[N_{j,k}]$, and $C_{j,k}$. Because we observe a single set of location decisions for each (j, k) pair, we use an econometric model to estimate social interaction.

For our econometric model, a natural starting point is the widely used approach of Bayer, Ross and Topa (2008), who propose an empirical strategy that uses excess variance to identify social interactions and exploits detailed geographic data, which we have. Extending their model to our setting yields

$$D_{i,j(i),k} D_{i',j(i'),k} = \alpha_{g,k} + \sum_{j \in g} \beta_{j,k} 1[j(i) = j(i') = j] + \epsilon_{i,i',k}, \tag{2.2}$$

where $j(i)$ is the birth town of migrant i, and both i and i' live in birth town group g. As described below, we define birth town groups in two ways: counties and square grids independent of county borders. The fixed effect $\alpha_{g,k}$ equals the average propensity of migrants from birth town group g to co-locate in destination k, while $\beta_{j,k}$ equals the additional propensity of individuals from the same birth town j to co-locate in k.[11] Equation (2.2) allows location decision determinants to vary arbitrarily at the birth town group-destination level through $\alpha_{g,k}$ (e.g., because of differences in migration costs due to railroad lines or highways).

[11] Bayer, Ross and Topa (2008) study the propensity of workers from the same census block to work together, beyond the propensity of workers from the same block group (a larger geographic area) to work together. Their outcome is binary: whether two individuals work in the same census block. In their initial specification, $\alpha_{g,k}$ does not vary by k, and $\beta_{j,k}$ does not vary by j or k. In other specifications, they allow the slope coefficient to depend on observed characteristics of the pair (i, i').

To better understand the reduced-form model in equation (2.2), we show how to map the parameters of the extended Bayer, Ross and Topa (2008) model, $(\alpha_{g,k}, \beta_{j,k})$, into classic parameters governing social interaction, $(P_{j,k}, C_{j,k})$. Doing so requires two assumptions. The most important assumption is that $P_{j,k}$ is constant across nearby birth towns in the same group:

Assumption 1. $P_{j,k} = P_{j',k}$ for different birth towns in the same birth town group, $j \neq j' \in g$.

Assumption 1 formalizes the idea that there are no ex-ante differences across nearby birth towns in the value of moving to destination k. For example, this assumes away the possibility that migrants from Pigeon Creek, Alabama had preferences or human capital particularly suited for Niagara Falls, New York relative to migrants from a nearby town, such as Oaky Streak, which was 6 miles away. This assumption attributes large differences in realized moving propensities across nearby towns to social interactions. Assumption 1 covers the probability of choosing a destination, conditional on migrating; we make no assumptions regarding out-migration probabilities.

Assumption 1 is plausible in our setting. Preferences for destination features (e.g., wages or climate) likely did not vary sharply across nearby birth towns. Potential migrants had little information about most destinations outside of what was provided through social networks. Furthermore, African Americans tended to work in different industries in the North and South, suggesting a negligible role for human capital specific to a birth town, destination county pair. The fixed effect $\alpha_{g,k}$ soaks up broader variation in human capital, such as the fact that some Great Plains migrants chose specific locations in California to pick cotton (Gregory, 1989). Conditional on out-migration, the cost of moving to a specific destination likely did not vary sharply across nearby towns.[12]

Importantly, Assumption 1 yields a testable prediction. This assumption relies on geographic proximity to control for the relevant determinants of location decisions. As a result, using observed birth town-level covariates to explain moving probabilities should not affect estimates of $P_{j,k}$ or our social interaction estimates. As discussed in detail below, we test this prediction and find evidence consistent with Assumption 1.

The second assumption is that social interaction occurs only among individuals from the same

[12] Assumption 1 is not violated if the cost of moving to all destinations varied sharply across birth towns (e.g., because of proximity to a railroad), as we focus on where people move, conditional on migrating.

birth town:

Assumption 2. $\mathbb{C}[D_{i,j,k}, D_{i',j',k}] = 0$ for individuals from different birth towns, $j \neq j'$.

Assumption 2 allows us to map the parameters of the extended Bayer, Ross and Topa (2008) model, $(\alpha_{g,k}, \beta_{j,k})$, into the key parameters governing social interaction, $(P_{j,k}, C_{j,k})$. Positive social interactions across nearby towns, which violates Assumption 2, would lead us to underestimate the strength of town-level social interactions, $\beta_{j,k}$.

Under Assumptions 1 and 2, the slope coefficient in equation (2.2) equals the covariance of location decisions from birth town j to destination k: $\beta_{j,k} = C_{j,k}$.[13] In addition, the fixed effect in equation (2.2) equals the squared moving probability: $\alpha_{g,k} = (P_{g,k})^2$, where $P_{g,k}$ is the probability of moving from birth town group g to destination k. This analysis demonstrates that the Bayer, Ross and Topa (2008) model uses the covariance of decisions to measure social interactions.

Simply extending the Bayer, Ross and Topa (2008) model, which they use to study a binomial outcome, to a multinomial-outcome setting could lead to incorrect inferences about the strength of social interactions. To see this, let $\mu_{j,k} \equiv \mathbb{E}[D_{i,j,k}|D_{i',j,k} = 1]$ be the probability that a migrant moves from birth town j to destination k, given a randomly chosen migrant from birth town j makes the same move. Slight manipulation of the definition of the covariance of location decisions, $C_{j,k}$, yields

$$C_{j,k} = P_{g,k} \left(\mu_{j,k} - P_{g,k} \right). \tag{2.3}$$

Equation (2.3) shows that variation in $C_{j,k}$ arises from two sources: the probability of moving to a destination $(P_{g,k})$ and the "marginal social interaction effect" $(\mu_{j,k} - P_{g,k})$. For example, $C_{j,k}$ could be large for a popular destination like Chicago because $P_{g,k}$ is large, even if $(\mu_{j,k} - P_{g,k})$

[13] Proof:

$$\begin{aligned}
\beta_{j,k} &= \mathbb{E}[D_{i,j(i),k} D_{i',j(i'),k} | j(i) = j(i') = j] - \mathbb{E}[D_{i,j(i),k} D_{i',j(i'),k} | j(i) \neq j(i')] \\
&= \mathbb{E}[D_{i,j(i),k} D_{i',j(i'),k} | j(i) = j(i') = j] - (\mathbb{E}[D_{i,j,k}])^2 \\
&= \mathbb{C}[D_{i,j,k}, D_{i',j,k}] = C_{j,k}
\end{aligned}$$

The first line follows directly from equation (2.2). The second line follows from Assumptions 1 and 2. The third line follows from the definition of covariance.

is small. For less popular destinations, $(\mu_{j,k} - P_{g,k})$ could be very large, but $C_{j,k}$ will be small if $P_{g,k}$ is sufficiently small. As a result, the covariance of location decisions, $C_{j,k}$, is not an attractive measure of social interactions in a multinomial setting.

To characterize the strength of social interactions for receiving and sending locations, we propose an intuitive social interactions (SI) index: the expected increase in the number of people from birth town j that move to destination county k when an arbitrarily chosen person i is observed to make the same move,

$$\Delta_{j,k} \equiv \mathbb{E}[N_{-i,j,k}|D_{i,j,k} = 1] - \mathbb{E}[N_{-i,j,k}|D_{i,j,k} = 0], \tag{2.4}$$

where $N_{-i,j,k}$ is the number of people who move from j to k, excluding person i. A positive value of $\Delta_{j,k}$ indicates positive social interactions in moving from j to k, while $\Delta_{j,k} = 0$ indicates the absence of social interactions.

The SI index ($\Delta_{j,k}$) features several attractive properties as a method of measuring social interactions. The SI index permits comparisons of social interactions across heterogeneous receiving and sending locations. In addition, the SI index is consistent with multiple behavioral models, which is valuable given uncertainty about the true behavioral model. For example, suppose that all migrants in town j form coalitions of size s, all members of a coalition move to the same destination, and all coalitions move independently of each other. In this case, the SI index for each destination k depends only on the behavioral parameter s ($\Delta_{j,k} = s - 1$), while the covariance of location decisions depends on additional parameters ($C_{j,k} = (s-1)P_{g,k}(1-P_{g,k})/(N_j - 1)$). Section 2.4.5 shows how to connect our SI index to the model of Glaeser, Sacerdote and Scheinkman (1996). Another attractive property of the SI index that we demonstrate below is that it can be estimated non-parametrically with increasingly available data. The SI index could be used to study social interactions for many outcomes besides location choices.

In Appendix B.1, we show that the SI index, $\Delta_{j,k}$, can be expressed as

$$\Delta_{j,k} = \frac{(\mu_{j,k} - P_{g,k})(N_j - 1)}{1 - P_{g,k}} = \frac{C_{j,k}(N_j - 1)}{P_{g,k} - P^2_{g,k}}. \tag{2.5}$$

Several features of equation (C.16) are noteworthy. First, the SI index depends on the classic parameters governing social interaction, $(P_{g,k}, C_{j,k})$. Second, the SI index increases in the marginal social interaction effect, $(\mu_{j,k} - P_{g,k})$. If migrants move independently, then $\mu_{j,k} - P_{g,k} = \Delta_{j,k} = 0$. Third, the SI index does not necessarily increase in the number of migrants from birth town j, N_j, as the marginal social interaction effect might decrease in N_j.[14]

2.3.3 Estimating the Social Interactions Index

As suggested by equation (C.16), estimation of the SI index is straightforward. We first define birth town groups, and then non-parametrically estimate the underlying parameters $P_{g,k}$, $P^2_{g,k}$, and $C_{j,k}$.

We consider two ways of defining birth town groups. Our preferred approach balances the inclusion of very close towns, for which Assumption 1 likely holds, with the inclusion of towns that are further away and lead to a more precise estimate of $P_{g,k}$. We divide each birth state into a grid of squares with sides x^* miles long and choose x^* for each state using cross validation.[15] Given x^*, the location of the grid is determined by a single latitude-longitude reference point.[16] Results

[14] In addition, $-1 \leq \Delta_{j,k} \leq N_j - 1$. At the upper bound, all migrants from j move to the same location, while at the lower bound, migrants displace each other one-for-one.

[15] That is,

$$x^* = \arg\min_x \sum_j \sum_k \left(N_{j,k}/N_j - \hat{P}_{g(x),-j,k}\right)^2,$$

where $\hat{P}_{g(x),-j,k} = \sum_{j' \neq j \in g(x)} N_{j',k} / \sum_{j' \neq j \in g(x)} N_{j'}$ is the average moving propensity from the birth town group of size x, excluding moves from town j. If there is only one town within a group g, then we define $\hat{P}_{g(x),-j,k}$ to be the statewide moving propensity. We search over even integers for convenience.

[16] In a related but substantively different setting, Billings and Johnson (2012) use cross validation in estimating the degree of industrial specialization. Duranton and Overman (2005) and Billings and Johnson (2012) estimate specialization parameters that do not require the aggregation of decisions at a spatial level. In contrast, we aggregate decisions at the receiving and sending county level. Doing so allows us to examine whether observed economic characteristics are related with patterns of social interactions.

are very similar across four different reference points, so we average estimates across them.

An alternative definition of a birth town group is a county. If the value of choosing a given destination varies sharply with county borders in the sending region, then this definition is appropriate. Differences across counties, such as local government policies, do not necessarily imply that counties are better birth town groups than those constructed with cross validation; what matters is whether these differences affect the probability of choosing a destination, conditional on migrating. An important advantage of using cross-validation is that it facilitates comparisons across birth states, which differ widely in average county size. We emphasize results based on cross validation in the main text and include results based on counties as birth town groups in the appendix.[17]

We estimate the probability of moving from birth town group g to destination county k as the total number of people who move from g to k divided by the total number of migrants in g,

$$\widehat{P_{g,k}} = \frac{\sum_{j \in g} N_{j,k}}{\sum_{j \in g} N_j}. \tag{2.6}$$

We estimate the squared moving probability using the closed-form solution implied by equation (2.2),[18]

$$\widehat{P^2_{g,k}} = \frac{\sum_{j \in g} \sum_{j' \neq j \in g} N_{j,k} N_{j',k}}{\sum_{j \in g} \sum_{j' \neq j \in g} N_j N_{j'}}, \tag{2.7}$$

and the covariance of location decisions using the closed-form solution implied by equation (2.2),

$$\widehat{C_{j,k}} = \frac{N_{j,k}(N_{j,k} - 1)}{N_j(N_j - 1)} - \widehat{P^2_{g,k}}. \tag{2.8}$$

The final component of the SI index is the number of migrants from birth town j, N_j.

Given $(\widehat{P_{g,k}}, \widehat{P^2_{g,k}}, \widehat{C_{j,k}}, N_j)$, we can estimate the SI index, $\Delta_{j,k}$, using equation (C.16). How-

[17] Appendix Figures B.2 and B.3 describe the number of birth towns per group when groups are defined using cross validation for Southern black and Great Plains white migrants. All groups used in estimation have at least two towns in them, and the median number of towns per group is 15 for African Americans and 39 for whites from the Great Plains. Appendix Figures B.4 and B.5 describe the number of towns per county.

[18] Equation (2.7) yields an unbiased estimate of $P^2_{j,k}$ under Assumptions 1 and 2. In contrast, simply squaring $\widehat{P_{g,k}}$ would result in a biased estimate.

ever, each estimate $\hat{\Delta}_{j,k}$ depends primarily on a single birth town observation. To conduct inference, increase the reliability of our estimates, and decrease the number of parameters reported, we aggregate SI index estimates across all birth towns in a given state for each destination county,

$$\hat{\Delta}_k = \sum_j \left(\frac{\widehat{P_{g(j),k}} - \widehat{P^2_{g(j),k}}}{\sum_{j'} \widehat{P_{g(j'),k}} - \widehat{P^2_{g(j'),k}}} \right) \hat{\Delta}_{j,k}, \tag{2.9}$$

where $g(j)$ is the group of town j. The destination level SI index estimate, $\hat{\Delta}_k$, is robust to small estimates of $P_{g,k}$, which can blow up estimates of $\Delta_{j,k}$. The weighting scheme used in equation (2.9) arises naturally from assuming that $\Delta_{j,k}$ does not vary across birth towns within a state.[19] The destination level SI index estimate, $\hat{\Delta}_k$, allows us to identify the destinations for which social interactions were particularly important and the economic characteristics associated with stronger social interactions.

We also construct birth county level SI index estimates by aggregating across destinations and towns within a birth county,

$$\hat{\Delta}_c = \sum_k \sum_{j \in c} \left(\frac{\widehat{P_{g(j),k}} - \widehat{P^2_{g(j),k}}}{\sum_{k'} \sum_{j' \in c} \widehat{P_{g(j'),k'}} - \widehat{P^2_{g(j'),k'}}} \right) \hat{\Delta}_{j,k}. \tag{2.10}$$

Birth county level SI index estimates have similar conceptual and statistical properties as destination county level SI index estimates.

To facilitate exposition, we have described estimation of the SI index in terms of four distinct components, $(\widehat{P_{g,k}}, \widehat{P^2_{g,k}}, \widehat{C_{j,k}}, N_j)$. In fact, the SI index estimates depend only on observed population flows, and equation (2.9) forms the basis of an exactly identified generalized method of moments (GMM) estimator. To estimate the variance of $\hat{\Delta}_k$, we treat the birth town group as the unit of observation and use a standard GMM variance estimator. This is akin to calculating standard errors clustered at the birth town group level.[20] Appendix B.2 contains details.

[19] When assuming $\Delta_{j,k} = \Delta_k \forall j$, the derivation in Appendix B.1 yields $\Delta_k = \left(\sum_j C_{j,k}(N_j - 1)\right) / \left(\sum_j P_{g(j),k}(1 - P_{g(j),k})\right)$, which leads directly to the estimator in equation (2.9).

[20] Treating birth town groups as the units of observation has no impact on the point estimate, $\hat{\Delta}_k$. We cluster because

2.3.4 An Extension to Assess the Validity of Our Empirical Strategy

The key threat to our empirical strategy is that the ex-ante value of moving to some destination differs across nearby birth towns in the same birth town group. If, contrary to this threat, Assumption 1 were true, then geographic proximity adequately controls for the relevant determinants of location decisions, and using observed birth town-level covariates to explain moving probabilities will not affect SI index estimates.

To assess this threat, we allow moving probabilities to depend on town level covariates,

$$P_{j,k} = \rho_{g,k} + X_j \pi_k, \tag{2.11}$$

where $\rho_{g,k}$ is a birth town group by destination fixed effect, and X_j is a vector of town level covariates whose effect on the moving probability can differ across destinations. We include in X_j an indicator for being along a railroad, an indicator for having above-median black population share, and four indicators corresponding to population quintiles.[21] These covariates, available from the Duke SSA/Medicare data and the railroad information used in Black et al. (2015*a*), capture potentially relevant determinants of location decisions. For example, migrants born in larger towns might have had more human capital or information and used these advantages to locate in certain destinations, and so our SI index estimates might reflect the role of birth town population size instead of social interactions; if this were the case, then our SI index estimates would be attenuated when controlling for population size. Equation (2.11) implies an alternative moving probability estimate, $\widetilde{P_{j,k}}$, as fitted values from the OLS regression

$$\frac{N_{j,k}}{N_j} = \rho_{g,k} + X_j \pi_k + e_{j,k}. \tag{2.12}$$

We use fitted values from a separate OLS regression, also implied by equation (2.11), to form

the estimates $\widehat{P_{g,k}}$ and $\widehat{P^2_{g,k}}$ are common to all birth towns within g.

[21] Percentiles are constructed separately for each birth state.

an alternative squared moving probability estimate, $\widetilde{P^2_{j,k}}$.[22] We estimate all equations separately by birth state. Our extended model uses these alternative estimates of $P_{j,k}$ and $P^2_{j,k}$ to construct alternative SI index estimates.[23] To the extent that the original and alternative SI index estimates are similar, this procedure provides support for our empirical strategy.[24]

2.4 Results: Social Interactions in Location Decisions

2.4.1 Social Interactions Index Estimates

Table 2.1 provides an overview of the long-run population flows that we use to estimate social interactions. Our data contain 1.3 million African Americans born in the South from 1916-1936, 1.9 million whites born in the Great Plains, and 2.6 million whites born in the South. In old age, 42 percent of Southern-born blacks and 35 percent of Great Plains-born whites lived outside their birth region, while only 9 percent of Southern-born whites lived outside the South.[25] As previously mentioned, we focus on Southern-born blacks and Great Plains-born whites in the main text, and leave results for Southern-born whites for the appendix. Appendix Table B.1 shows that, on average, there were 142 migrants per birth town for African Americans from the South, and 181 migrants per birth town for whites from the Great Plains.

We begin with some examples to illustrate how we identify social interactions in location decisions. Table 2.2 shows the birth town to destination county migration flows that would be most unlikely in the absence of social interactions. Panel A shows that, among these examples, 10-50

[22] We estimate $\widetilde{P^2_{j,k}}$ using fitted values from the OLS regression

$$\frac{N_{j,k}}{N_j}\frac{N_{j',k}}{N_{j'}} = \rho_{g(j),k}\rho_{g(j'),k} + X_j\pi_k\rho_{g(j'),k} + X_{j'}\pi_k\rho_{g(j),k} + (X_j\pi_k)(X_{j'}\pi_k) + e'_{j,j',k}$$

for different birth towns, $j \neq j'$.

[23] When including covariates, we ignore the variance from estimates of equation (2.11). Including this variance would make our estimates with and without covariates appear even more similar when performing statistical tests.

[24] An alternative approach to assessing the validity of Assumption 1 is testing whether the parameter vector $\pi_k = 0$ in equation (2.12). We prefer to test the difference in SI index estimates because this approach allows us to assess the statistical and substantive significance of any differences.

[25] Census data show that return migration was quite low among Southern-born blacks and much higher among Southern-born whites (Gregory, 2005).

percent of African-American migrants from each birth town lived in the same destination county in old age, while typically less than one percent of migrants from each birth state lived in the same county. The observed moving propensities are 50-65 standard deviations larger than what would be expected if individuals moved independently of each other according to the statewide moving propensities. The estimated moving probabilities, $\hat{P}_{j,k}$, exceed the statewide moving propensities, suggesting a meaningful role for local conditions in determining location decisions. Most importantly, the observed moving propensities are much larger than the estimated moving probabilities, consistent with a positive estimated covariance of location decisions, and ultimately, positive SI index estimates. The results in Panel B for Great Plains whites are similar.

To summarize the importance of social interactions for all location decisions in our data, Table 2.3 reports averages of destination level SI index estimates. Our data contain 516,712 black migrants from the South and 644,523 white migrants from the Great Plains.[26] For African Americans, unweighted averages of the destination level SI index, $\hat{\Delta}_k$, across all destination counties vary from 0.46 (Louisiana) to 0.90 (Mississippi), as seen in column 2. Weighted averages in column 3 vary from 0.81 (Florida) to 2.61 (South Carolina) and are larger because we generally estimate stronger social interactions in destinations that received more migrants. We prefer the weighted average as a summary measure because it better reflects the experience of a randomly chosen migrant and depends less on our decision to combine destination counties with fewer than 10 migrants. Across all states, the migrant-weighted average of destination level SI index estimates in column 3 is 1.94; this means that when we observe one randomly chosen African American move from a birth town to some destination, then on average 1.94 additional black migrants from that birth town would make the same move. Panel B presents results for white moves out of the Great Plains. The weighted average of destination level SI index estimates for whites is 0.38, only one-fifth the size of the average for African Americans.[27] These results indicate that African American migrants

[26] The number of migrants in Table 2.3 differs slightly from the implied number of migrants in Table 2.1 because we exclude individuals from birth towns with fewer than 10 migrants when we estimate the SI index.

[27] Appendix Table B.2 shows that results are similar when we define birth town groups using counties. For Southern blacks, the linear (rank) correlation between the destination level SI index estimates using cross validation and counties is 0.858 (0.904). For whites from the Great Plains, the linear (rank) correlation is 0.965 (0.891). Appendix Table B.3 shows that average SI index estimates for whites from the South are small.

relied more heavily on social networks in making their long-run location decisions. Given the historical context, one explanation for this finding is that African Americans used social networks to overcome their lack of resources or the discrimination they faced in many destinations.

We provide a more complete picture of social interactions in Figure 2.4, which plots the distributions of destination level SI index estimates.[28] The figure shows that social interactions were particularly strong for some destinations and relatively weak for most destinations. As described below, our empirical approach allows us to examine whether this considerable heterogeneity can be explained by destinations' economic characteristics.[29] Across the board, SI index estimates for African Americans are larger than those for whites.

To examine social interactions more closely, Figure 2.5 plots the spatial distribution of destination level SI index estimates for Mississippi-born blacks. There is evidence of strong social interactions in many Northern destinations: 23 counties have an estimated SI index greater than 3 and 58 counties have an estimated SI index between 1 and 3. These counties lie in the Midwest and, to a lesser degree, the Northeast. The figure also shows that African Americans moved to a relatively small number of destination counties, consistent with limited opportunities, information, or interest in moving to many places in the U.S.[30] We estimate particularly strong social interactions ($\hat{\Delta}_k > 3$) in Rock County, Wisconsin, which contains Beloit, consistent with historical accounts suggesting strong social interactions for Mississippi-born African Americans in Beloit (Bell, 1933; Rubin, 1960; Wilkerson, 2010). Figure 2.6 maps the destination level SI index estimates for whites from North Dakota. We find little evidence of strong social interactions, although one exception is San Joaquin county ($\hat{\Delta}_k > 3$), an area described memorably in the novel *The Grapes of Wrath* (Steinbeck, 1939).[31] In contrast to black migrants, whites moved to a large number of destinations throughout the U.S. The difference between the number of destinations chosen

[28] A single destination county can appear multiple times in these figures because we estimate destination level SI indices separately for each birth state.

[29] Appendix Figure B.6 displays the associated t-statistic distributions, and Appendix Figures B.7 and B.8 display analogous results for whites from the South.

[30] In Figure 2.5, the counties in white received less than 10 migrants.

[31] In *The Grapes of Wrath*, the Joad family travels from Oklahoma to the San Joaquin Valley. Gregory (1989) notes that the (fictional) Joads were poorer than many migrants from the Great Plains.

by Mississippi blacks and North Dakota whites is striking, especially because there were more migrants from Mississippi (120,454 versus 92,205). Appendix Figures B.9 and B.10, for Southern Carolina-born blacks and Kansas-born whites, show similar patterns.

To assess the validity of our empirical strategy, we examine whether SI index estimates change when we use birth town level covariates to explain moving probabilities. Under our key identifying Assumption 1, geographic proximity adequately controls for the relevant determinants of location decisions, and so additional covariates should have no impact. Table 2.4 reports weighted averages of destination level SI index estimates with and without covariates. When we examine birth states individually, there are no substantively or statistically significant differences between the two sets of estimates. When pooling all Southern states together, the estimates are very similar in magnitude (1.94 and 1.92) and statistically indistinguishable ($p = 0.76$). When pooling all Great Plains states together, the estimates again are very similar in magnitude (0.38 and 0.36), but are statistically distinguishable ($p = 0.02$). In addition, the destination level SI index estimates with and without covariates are highly correlated: the linear (rank) correlation is 0.914 (0.992) for blacks from the South and 0.939 (0.988) for whites from the Great Plains. On net, this evidence suggests that geographic proximity adequately controls for the relevant determinants of location decisions and supports the validity of our empirical strategy.

To examine the robustness of our results and a potentially important dimension of heterogeneity, we examine average SI index estimates that exclude migration from large birth towns and migration to large destination counties. Birth town size could be correlated with unobserved determinants of social interactions and location decisions, such as the level of social and human capital or information about destinations. Based on previous qualitative work and simple economic models, we expect substantial social interactions in small birth towns, but small towns need not feature stronger social interactions than large towns. Similarly, we expect substantial, but not necessarily larger, social interactions in smaller destination counties.

For reference, column 1 of Table 2.5 reports weighted averages of destination level SI index estimates when including all birth towns and destinations. In column 2, we exclude birth towns with at least 20,000 residents in 1920 when estimating each destination level SI index.[32] Column 3 excludes destination counties that intersect with the ten largest non-South consolidated metropolitan statistical areas (CMSAs) as of 1950, in addition to counties that received less than 10 migrants.[33] We exclude both large birth towns and large destinations in column 4. The average SI index estimates are similar across all four specifications for both Southern blacks and Great Plains whites.[34] In sum, this table shows that our results are not driven by migration from the largest birth towns or migration to the largest destinations and, relatedly, that there is limited heterogeneity in SI index estimates on these dimensions.

One of the most widely noted features of the Great Migration is the tendency of migrants to move along vertical pathways established by South-to-North railroad lines. In effect, railroads reduced the cost of moving to a Northern destination on the same line and increased the flow of information. Social interactions might not have followed this pattern if they drew migrants to destinations that they would not consider otherwise. However, social interactions could have been fostered by the reduced migration costs and increased information that generated vertical migration patterns. To examine this, Table 2.6 displays weighted averages of destination level SI index estimates for different regions.[35] Social interactions among African Americans clearly follow vertical migration patterns: the largest SI index estimates in the Northeast come from the Carolinas, while the largest estimates in the Midwest are among migrants from Mississippi and Alabama, and the largest estimates in the West come from Louisiana.[36] Panel B displays weighted

[32]These birth towns are Birmingham, Mobile, and Montgomery, Alabama; Jacksonville, Miami, Pensacola, and Tampa, Florida; Atlanta, Augusta, Columbus, Macon, and Savannah, Georgia; Baton Rouge, New Orleans, and Shreveport, Louisiana; Jackson and Meridian, Mississippi; Asheville, Charlotte, Durham, Raleigh, Wilmington, and Winston-Salem, North Carolina; Charleston, Greenville, and Spartanburg, South Carolina; Hutchinson, Kansas City, Topeka, and Wichita, Kansas; Lincoln and Omaha, Nebraska; Fargo, North Dakota; Muskogee, Oklahoma City, and Tulsa, Oklahoma; Sioux Falls, South Dakota

[33]The ten CMSAs are New York, Chicago, Los Angeles, Philadelphia, Boston, Detroit, Washington, D.C., San Francisco, Pittsburgh, and St. Louis. The first nine of these are also the largest non-Great Plains (and border region) CMSAs; our sample of Great Plains migrants does not include individuals who moved to St. Louis because Missouri is in the border region.

[34]Appendix Table B.4 reports similar results for Southern-born whites.

[35]Appendix Table B.5 reports regional results for Southern-born whites.

[36]The Northeast region includes Connecticut, Delaware, Washington, D.C., Maine, Maryland, Massachusetts, New Hampshire, New Jersey, New York, Pennsylvania, Rhode Island, Vermont, and West Virginia. The Midwest region includes Illinois, Indiana, Iowa, Kansas, Kentucky, Michigan, Minnesota, Missouri, Nebraska, North Dakota, Ohio, Oklahoma, South Dakota, and Wisconsin. The West region includes Alaska, Arizona, California, Colorado, Hawaii,

averages by region for Great Plains whites. Social interactions among Great Plains whites were much stronger in the Midwest and West, where moving costs were lower, than the Northeast or South. These patterns suggest that lower migration costs and greater information facilitated social interactions.

To further understand the nature of social interactions, we examine whether the location decisions of African American migrants influenced the location decisions of white migrants from the same Southern birth town, and vice versa. While, in principle, whites and blacks could have shared information about opportunities in the North, the level of segregation in the Jim Crow South makes cross-race social interactions unlikely. Appendix B.3 provides details on how we estimate cross-race social interactions, and Appendix Table B.6 provides little evidence of cross-race interactions. These results demonstrate that social interactions operated within racial groups. In addition, there is little correlation between destination level SI index estimates for blacks and whites from the South: the linear (rank) correlation is 0.076 (0.149). This implies that our SI index estimates do not simply reflect unobserved characteristics of certain Southern towns.

2.4.2 Addressing Measurement Error due to Incomplete Migration Data

SI index estimates depend on measured population flows, which are incomplete because some individuals die before enrolling in Medicare and some individuals' birth town information is unavailable. We first address the implications of measurement error due to incomplete migration data under a missing at random assumption. If we observe a random sample of migration flows for each birth town-destination combination, then measurement error does not bias estimates of the covariance of location decisions, $C_{j,k}$, or moving probabilities, $P_{j,k}$. As a result, equation (C.16) shows that our SI index estimates will be attenuated because we undercount the number of migrants, N_j.

More specifically, suppose that we are interested in the effect of social interactions on location decisions at age 40. Denote the number of migrants that survive to age 40 by N_j^{40}, and

Idaho, Montana, Nevada, New Mexico, Oregon, Utah, Washington, and Wyoming. The South region includes Alabama, Arkansas, Florida, Georgia, Louisiana, Mississippi, North Carolina, South Carolina, Tennessee, Texas, and Virginia. These regions vary from Census-defined regions because we define the South to be the Confederacy.

assume for simplicity that this equals the observed number of migrants divided by a scaling factor, $N_j^{40} = N_j/\alpha$. To approximate the coverage rate α, we divide the number of individuals in the Duke/SSA Medicare data by the number of individuals in decennial census data.[37] Across birth states, the average coverage rate is 52.5% for African Americans from the South and 69.7% for whites from the Great Plains (see Appendix Table B.7), which implies that $N_j^{40} \approx 1.90N_j$ for Southern blacks and $N_j^{40} \approx 1.43N_j$ for Great Plains whites. As an approximate measurement error correction, SI index estimates should be multiplied by a factor of 1.90 for Southern blacks and 1.43 for Great Plains whites. Appendix Table B.8 presents results that reflect state-specific coverage rate adjustments. The weighted average of destination level SI index estimates is 3.69 for Southern blacks and 0.56 for Great Plains whites. Adjusting for incomplete data under a missing at random assumption increases the magnitude of SI index estimates and increases the gap between black and white social interaction estimates.

Appendix B.4 describes the implications of measurement error when we relax the missing at random assumption. We derive a lower bound on the social interactions (SI) index and show that estimates of this lower bound still reveal sizable social interactions.

2.4.3 The Role of Family Migration

If migrants relied on family members from the same birth town when making their location decisions, then our SI index would reflect this behavior, as it should. While family migration does not represent a threat to our results, it would be interesting to know the extent to which social interactions occur within the family. Unfortunately, we do not have information on family membership and are limited in our ability to address this issue directly. We can examine whether our results stem entirely from the migration of heterosexual couples. If this were true, there would be no social interactions among men only or women only. We find that SI index estimates are similar in magnitude among men and women (see Appendix Table B.8), and we conclude that our

[37] We use the 1960 Census to construct coverage rates for individuals born from 1916-1925 and the 1970 Census for individuals born from 1926-1935.

results do not simply reflect the migration of couples.[38] Our sample likely contains very few sets of parents and children, since we only include individuals born from 1916-1936.

A related question is the extent to which differences in family structure explain differences in social interactions between black and white migrants. As a first step to providing evidence on this question, we use the 1940 Census to measure the average within-household family size for individuals born from 1916-1936. African Americans from the South had families that were 17 percent larger than those of whites from the Great Plains (6.16 versus 5.25). This gap clearly does not explain our finding that average SI index estimates are 410 percent larger among blacks than whites.[39] To construct an upper bound on extended family size, we use the 100 percent sample of the 1940 Census to count the average number of individuals in a county born from 1916-1936 with the same last name (Minnesota Population Center and Ancestry.com, 2013). Southern black family networks likely were no more than 270 percent larger than those for Great Plains whites (54.5 versus 14.7). This upper bound is sizable, but still less than the 410 percent difference in social interaction strength. Differences in family structure might explain some, but not all, of the differences in social interactions between black and white migrants.

2.4.4 Social Interactions and Economic Characteristics of Receiving and Sending Locations

The results above show that social interactions were extremely important for the location decisions of African Americans and less important for whites. They also show that the strength of social interactions varied considerably across space. To better understand why social interactions affected location decisions, we relate estimates of the SI index to economic characteristics of receiving and sending locations. We focus on African American migrants in the text because social interactions were more important for this group and present results for white migrants in the appendix.

[38]The similarity between men and women is not surprising given the relative sex balance among migrants in this period (Gregory, 2005).

[39]The weighted average of SI index estimates in Table 2.3 is 1.938 for blacks and 0.380 for whites, and (1.938-0.380)/0.380 = 4.1. When adjusting for incomplete migration data under the missing at random assumption, social interactions among African Americans are 559 percent larger than among Great Plains whites.

We begin by considering the economic characteristics of receiving locations. Employment opportunities were among the most important features of a destination, and employment in the manufacturing sector was particularly attractive to African Americans because of its relatively high wages and demand for workers. In the presence of imperfect information, networks might have directed their members to destinations with more manufacturing employment. This is the story of John McCord. Because migrants almost certainly had more information about employment opportunities in the largest destinations, the imperfect information channel suggests a stronger relationship between social interactions and manufacturing employment intensity in small destinations. However, if information about employment opportunities was widely known, then social interactions might not be stronger in destinations with more manufacturing. Pecuniary moving costs, which were largely determined by railroads and physical distance, represented another key economic characteristic of destinations. Lower moving costs could have fostered social interactions by facilitating the transmission of information. On the other hand, migrants might have been willing to travel to high moving cost destinations only if they received information or benefits from a network there.

To explore these hypotheses, we regress destination level SI index estimates on county level covariates. Column 1 of Table 2.7 shows that social interactions were significantly larger in destinations with a higher 1910 manufacturing employment share: a one standard deviation increase in the 1910 manufacturing employment share is associated with a 12 percent increase in the SI index at the mean.[40] Column 2 shows that the positive relationship between manufacturing employment and social interactions was over twice as large in smaller destinations.[41] We also find that social interactions were significantly stronger in destinations that were closer to the birth state. However, there is no relationship between the strength of social interactions and whether a destination could be reached directly or with one-stop by rail from the birth state. One possible concern is

[40] We report summary statistics in Appendix Table B.9. Appendix Figure B.11, which plots the bivariate relationship between social interaction estimates and 1910 manufacturing employment share, shows the considerable variation in the manufacturing employment share across destinations.

[41] Small destination counties are those that do not intersect with the ten largest non-South CMSAs in 1950 (New York, Chicago, Los Angeles, Philadelphia, Boston, Detroit, Washington, D.C., San Francisco, Pittsburgh, and St. Louis).

that these results do not reflect characteristics of destination counties, but instead characteristics of birth states. As seen in column 3, the data do not support this concern: adding birth state fixed effects has very little impact.[42]

In sum, the results in Table 2.7 suggest that migrants relied on social networks to overcome imperfect information about employment opportunities, and that migrants had less non-network information about smaller destinations. The results also suggest that low moving costs facilitated social interactions.

We next consider the relationship between social interactions and the economic characteristics of sending counties. Social networks could have been particularly important in locating jobs or housing for migrants from poorer communities who had fewer resources to engage in costly search. Another salient characteristic of sending locations was the share of the population in rural areas. Rural areas might have had less non-network information about destinations, making networks more valuable. Alternatively, social ties in rural areas might have been weaker due to the lower population density there (Chay and Munshi, 2015). We also characterize counties' exposure to Rosenwald schools, which improved educational attainment among Southern blacks in this period (Aaronson and Mazumder, 2011). The relationship between human capital attainment and social interaction is unclear, as human capital could promote social ties in the South while also increasing the relative return to choosing a non-network destination. In addition, we examine whether social interactions were stronger in counties with greater access to railroads, which could have facilitated the transmission of information through both network and non-network channels.

Table 2.8 displays results from regressing birth county level SI index estimates on birth county characteristics. Social interactions were stronger in poorer counties, measured as the share of residents with income less than $2,000 in 1950. The point estimate on the rural population share is negative, but only significant when including birth state fixed effects in column 2. A one standard deviation increase in the share of poor residents is associated with a 41 percent increase in the SI index at the mean, while a one standard deviation increase in the percent rural is associated

[42] Results are qualitatively similar using counties to define birth town groups (Appendix Table B.10). Results for Great Plains whites and Southern whites are in Appendix Tables B.11 and B.12.

with a 46 percent decrease in social interactions. We find little evidence that social interactions varied with Rosenwald school or railroad exposure, though the standard errors are fairly large. In both specifications in Table 2.8, we control for the log number of migrants from a sending county to ensure that our results do not spuriously reflect out-migration patterns. In sum, we find that migrants from poorer communities relied more heavily on social networks in their location decisions. This is consistent with networks providing several possible benefits, such as reducing the time required to find a job or affordable housing.

2.4.5 Connecting the Social Interactions Index to a Behavioral Model

The results above rely on estimates of the SI index developed in this paper. Next, we connect the SI index to the behavioral model of social interactions from Glaeser, Sacerdote and Scheinkman (1996). The assumptions in their model allow us to estimate the share of migrants that chose their long-run location because of social interactions, a parameter that complements our SI index in intuitively describing the size of social interactions. This connection also demonstrates that our SI index can be used to integrate the behavioral model of Glaeser, Sacerdote and Scheinkman (1996) and the general identification strategy of Bayer, Ross and Topa (2008).

Migrants, indexed on a line by $i \in \{1, \ldots, N_j\}$, are either a "fixed agent" or a "complier." A fixed agent chooses her location independently of other migrants. If i is a complier, then he chooses the same destination as his neighbor, $i-1$. The probability that an individual is a complier equals χ, assumed for simplicity to be constant across birth towns and destinations for a given birth state. The covariance of location decisions for individuals i and $i+n$ is $\mathbb{C}[D_{i,j,k}, D_{i+n,j,k}] = P_{g,k}(1-P_{g,k})\chi^n$. Hence, the average covariance of location decisions implied by the model is

$$C_{j,k}(\chi; P_{g,k}, N_j) \equiv \frac{\sum_{i \in j} \sum_{i' \neq i \in j} \mathbb{C}[D_{i,j,k}, D_{i',j,k}]}{N_j(N_j-1)} \tag{2.13}$$

$$= \frac{2P_{g,k}(1-P_{g,k}) \sum_{s=1}^{N_j-1} (N_j - s)\chi^s}{N_j(N_j-1)}. \tag{2.14}$$

In the absence of social interactions, there are no compliers, and the covariance of location decisions equals zero.[43]

Substituting the expression for $C_{j,k}(\chi; P_{g,k}, N_j)$ in equation (2.14) into the expression for the SI index, $\Delta_{j,k}$, in equation (C.16) yields

$$\Delta_{j,k} = 2 \sum_{s=1}^{N_j - 1} (1 - s/N_j)\chi^s. \tag{2.15}$$

With a sufficiently large number of migrants, we obtain $\Delta_{j,k} = 2\chi/(1-\chi)$. Because the destination level SI index, Δ_k, is just a weighted average of the SI index, $\Delta_{j,k}$, and the average destination level SI index, denoted Δ, is just a weighted average of Δ_k, we can estimate the probability that an individual is a complier as

$$\hat{\chi} = \frac{\hat{\Delta}}{2 + \hat{\Delta}}. \tag{2.16}$$

As seen in Table 2.9, we estimate that between 29 (Florida) and 57 percent (South Carolina) of black migrants chose their long-run location because of social interactions. There is considerable variation across destination regions.[44] For example, of Mississippi-born migrants, 32 percent of Northeast-bound, 57 percent of Midwest-bound, and 34 percent of West-bound migrants chose their location because of social interactions. Among whites from the Great Plains, between 11 (Kansas) and 19 percent (North Dakota) of migrants chose their destination because of social interactions. Although these estimates depend on stronger assumptions than are necessary to estimate our SI index, they help illustrate the considerable impact of social interactions on location decisions for Southern blacks and the smaller impact among whites.

[43]Glaeser, Sacerdote and Scheinkman (1996) measure social interactions using the normalized variance of outcomes, which in our model is

$$\mathbb{V}\left[\sum_{i=1}^{N_j} \frac{D_{i,j,k} - P_{g,k}}{N_j}\right] = \frac{P_{g,k}(1 - P_{g,k})}{N_j} + \left(\frac{N_j - 1}{N_j}\right) C_{j,k}(\chi; P_{g,k}, N_j).$$

[44]Assuming that χ is constant across destinations implies that it should not vary across different regions. Nonetheless, we find the rescaled regional estimates to be informative. Appendix B.5 contains a richer model that allows the probability of complying to vary with birth town and destination.

2.5 Conclusion

This paper provides new evidence on the magnitude and nature of social interactions in location decisions. We use confidential administrative data to study over one million long-run location decisions made by African Americans born in the U.S. South and whites born in the Great Plains during two landmark migration episodes. We formulate a novel social interactions (SI) index that characterizes the strength of social interactions for each receiving and sending location, which allows us to estimate not only the overall magnitude of social interactions, but also the degree to which social interactions were associated with economic characteristics of receiving and sending locations. The SI index can be used for other outcomes and settings to provide a deeper understanding of social interactions in economic outcomes.

We find very strong social interactions among Southern black migrants and smaller interactions among whites. Estimates of our social interactions (SI) index imply that if we observed one randomly chosen African American move from a birth town to some destination, then on average 1.9 additional black migrants from that birth town would make the same move. For white migrants from the Great Plains, the average is only 0.4, and results for Southern whites are similarly small. Interpreted through the social interactions model of Glaeser, Sacerdote and Scheinkman (1996), our estimates imply that 49 percent of African-American migrants chose their long-run destination because of social interactions, while 16 percent of Great Plains whites were similarly influenced. One interpretation of our results is that African Americans relied on social networks more heavily to overcome the more intense discrimination they faced in labor and housing markets. In addition, our results suggest that social interactions were particularly important in providing African American migrants with information about attractive employment opportunities in smaller destinations, and that social interactions played a larger role in less costly moves. Our results also suggest that migrants from poorer sending communities relied more heavily on social interactions.

Our results shed new light on how individuals decide where to move. Social interactions are of first-order importance in our setting, especially for migrants with the fewest resources and opportunities. Our results suggest that social interactions help migrants address the substantial information frictions that characterize long-distance location decisions. Social interactions likely play an important role in the rural-to-urban migration taking place across the developing world, and policies that seek to direct migration to certain areas should account for the role of social interactions.

Our results also have implications for economic outcomes in the U.S. during the twentieth century. Birth town social networks continued to operate after location decisions had been made, and the Great Migration generated considerable variation in the strength of social networks across destinations. In ongoing work, we use this variation to study the relationship between crime and social capital in Northern cities (Stuart and Taylor, 2014). Examining the impacts of social capital on other economic outcomes in destination cities is a promising direction for future work.

Table 2.1: Location at Old Age, 1916-1936 Cohorts

		Percent Living in Location		
		Outside Birth	In Birth Region	
	People	Region	Birth State	Other State
Birth State	(1)	(2)	(3)	(4)
Panel A: Southern Blacks				
Alabama	209,128	47.2%	39.5%	13.3%
Florida	79,237	26.1%	67.1%	6.8%
Georgia	218,357	36.3%	44.2%	19.5%
Louisiana	179,445	32.4%	52.7%	14.9%
Mississippi	218,759	56.1%	28.9%	15.0%
North Carolina	200,999	40.2%	49.7%	10.1%
South Carolina	163,650	43.4%	41.9%	14.7%
Total	1,269,575	41.8%	44.0%	14.1%
Panel B: Southern Whites				
Alabama	469,698	9.8%	62.1%	28.1%
Florida	231,071	12.7%	68.5%	18.8%
Georgia	454,286	7.4%	65.5%	27.1%
Louisiana	384,601	8.7%	71.1%	20.2%
Mississippi	275,147	11.0%	57.0%	32.0%
North Carolina	588,674	8.5%	71.6%	19.8%
South Carolina	238,697	6.6%	70.6%	22.8%
Total	2,642,174	9.0%	66.9%	24.0%
Panel C: Great Plains Whites				
Kansas	462,490	30.4%	43.3%	26.3%
Nebraska	374,265	36.0%	42.0%	22.0%
North Dakota	210,199	44.1%	31.8%	24.1%
Oklahoma	635,621	31.8%	41.6%	26.6%
South Dakota	196,266	40.4%	35.4%	24.2%
Total	1,878,841	34.6%	40.3%	25.1%

Notes: Column 1 contains the number of people from the 1916-1936 birth cohorts observed in the Duke SSA/Medicare data. Columns 2-4 display the share of individuals living in each location at old age (2001 or date of death, if earlier). Figure 2.3 displays birth regions. Southerners' birth region is the Confederacy. The Great Plains birth region includes the Plains and border states.

Source: Authors' calculations using Duke SSA/Medicare data

Table 2.2: Extreme Examples of Correlated Location Decisions, Southern Blacks and Great Plains Whites

Birth Town (1)	Largest City in Destination County (2)	Total Birth Town Migrants (3)	Town-Destination Flow (4)	Destination Share of Birth Town Migrants (5)	Destination Share of Birth State Migrants (6)	SD under Independent Binomial Moves (7)	Estimated Moving Probability $\hat{P}_{j,k}$ (8)	Social Interaction Estimate $\hat{\Delta}_k$ (9)
Panel A: Southern Blacks								
Pigeon Creek, AL	Niagara Falls, NY	85	43	50.6%	0.5%	64.5	4.5%	8.5
Marion, AL	Fort Wayne, IN	1311	200	15.3%	0.7%	63.7	3.8%	8.8
Greeleyville, SC	Troy, NY	215	34	15.8%	0.1%	62.2	1.7%	15.2
Athens, AL	Rockford, IL	649	64	9.9%	0.2%	61.0	2.0%	5.6
Pontotoc, MS	Janesville, WI	456	62	13.6%	0.2%	59.4	3.3%	6.5
New Albany, MS	Racine, WI	599	97	16.2%	0.4%	58.7	4.9%	11.4
West, MS	Freeport, IL	336	35	10.4%	0.1%	56.9	0.8%	6.2
Gatesville, NC	New Haven, CT	176	88	50.0%	1.6%	51.8	8.1%	7.1
Statham, GA	Hamilton, OH	75	22	29.3%	0.3%	50.0	3.0%	4.4
Cochran, GA	Paterson, NJ	259	62	23.9%	0.6%	49.4	4.1%	6.3
Panel B: Great Plains Whites								
Krebs, OK	Akron, OH	210	32	15.2%	0.1%	82.6	0.3%	7.4
Haven, KS	Elkhart, IN	144	22	15.3%	0.1%	51.1	0.4%	6.9
McIntosh, SD	Rupert, ID	299	20	6.7%	0.1%	50.9	0.6%	4.8
Hull, ND	Bellingham, WA	55	24	43.6%	0.5%	44.6	1.5%	4.3
Lindsay, NE	Moline, IL	226	29	12.8%	0.2%	41.5	0.4%	5.2
Corsica, SD	Holland, MI	253	26	10.3%	0.2%	39.6	0.4%	6.3
Corsica, SD	Grand Rapids, MI	253	34	13.4%	0.3%	37.2	0.7%	6.0
Montezuma, KS	Merced, CA	144	21	14.6%	0.3%	32.7	0.9%	2.7
Hillsboro, KS	Fresno, CA	407	65	16.0%	0.9%	32.0	1.2%	2.2
Henderson, NE	Fresno, CA	146	32	21.9%	0.7%	31.1	0.8%	2.2

Notes: Each panel contains the most extreme examples of correlated location decisions as determined by column 7. Column 7 equals the difference, in standard deviations, of the actual moving propensity (column 5) relative to the prediction with independent moves following a binomial distribution governed by the statewide moving propensity (column 6). Column 8 equals the estimated probability of moving from town j to county k using observed location decisions from nearby towns, where the birth town group is defined by cross validation. Column 9 equals the destination level social interaction estimate for the relevant birth state. When choosing these examples, we restrict attention to town-destination pairs with at least 20 migrants.
Source: Authors' calculations using Duke SSA/Medicare data

Table 2.3: Average Social Interactions Index Estimates, by Birth State

	Number of	Type of Average	
	Migrants	Unweighted	Weighted
Birth State	(1)	(2)	(3)
Panel A: Black Moves out of South			
Alabama	96,269	0.770	1.888
		(0.049)	(0.195)
Florida	19,158	0.536	0.813
		(0.052)	(0.117)
Georgia	77,038	0.735	1.657
		(0.048)	(0.177)
Louisiana	55,974	0.462	1.723
		(0.039)	(0.478)
Mississippi	120,454	0.901	2.303
		(0.050)	(0.313)
North Carolina	78,420	0.566	1.539
		(0.039)	(0.130)
South Carolina	69,399	0.874	2.618
		(0.054)	(0.301)
All States	516,712	0.736	1.938
		(0.020)	(0.110)
Panel B: White Moves out of Great Plains			
Kansas	139,374	0.128	0.255
		(0.007)	(0.024)
Nebraska	134,011	0.141	0.361
		(0.008)	(0.082)
North Dakota	92,205	0.174	0.464
		(0.012)	(0.036)
Oklahoma	200,392	0.112	0.453
		(0.008)	(0.036)
South Dakota	78,541	0.163	0.350
		(0.009)	(0.026)
All States	644,523	0.137	0.380
		(0.004)	(0.022)

Notes: Column 2 is an unweighted average of destination level social interaction estimates, $\hat{\Delta}_k$. Column 3 is a weighted average, where the weights are the number of people who move from each state to destination k. Birth town groups are defined by cross validation. Standard errors are in parentheses.
Source: Authors' calculations using Duke SSA/Medicare data

Table 2.4: Average Social Interactions Index Estimates, With and Without Controlling for Observed Differences across Birth Towns

	Control for Covariates		p-value of
	No	Yes	difference
Birth State	(1)	(2)	(3)
Panel A: Black Moves out of South			
Alabama	1.888	1.852	0.763
	(0.195)	(0.189)	
Florida	0.813	0.742	0.401
	(0.117)	(0.119)	
Georgia	1.657	1.689	0.658
	(0.177)	(0.175)	
Louisiana	1.723	1.651	0.862
	(0.478)	(0.474)	
Mississippi	2.303	2.295	0.967
	(0.313)	(0.306)	
North Carolina	1.539	1.482	0.149
	(0.130)	(0.127)	
South Carolina	2.618	2.636	0.827
	(0.301)	(0.304)	
All States	1.938	1.917	0.764
	(0.110)	(0.108)	
Panel B: White Moves out of Great Plains			
Kansas	0.255	0.233	0.112
	(0.024)	(0.024)	
Nebraska	0.361	0.349	0.504
	(0.082)	(0.082)	
North Dakota	0.464	0.445	0.456
	(0.036)	(0.035)	
Oklahoma	0.453	0.439	0.241
	(0.036)	(0.036)	
South Dakota	0.350	0.331	0.145
	(0.026)	(0.026)	
All States	0.380	0.363	0.021
	(0.022)	(0.022)	

Notes: All columns contain weighted averages of social interaction estimates, $\hat{\Delta}_k$, where the weights are the number of people who move from each state to destination k. Column 1 is identical to column 3 of Table 2.3. Column 2 controls for observed birth town covariates as described in the text. Column 3 reports the p-value from testing the null hypothesis that the two columns are equal. Birth town groups are defined by cross validation. Standard errors are in parentheses.
Source: Authors' calculations using Duke SSA/Medicare data

Table 2.5: Average Social Interactions Index Estimates, by Size of Birth Town and Destination

Exclude Largest Birth Towns	No	Yes	No	Yes
Exclude Largest Destinations	No	No	Yes	Yes
Birth State	(1)	(2)	(3)	(4)
Panel A: Black Moves out of South				
Alabama	1.888	1.784	2.056	2.189
	(0.195)	(0.149)	(0.285)	(0.268)
Florida	0.813	0.607	1.323	1.231
	(0.117)	(0.061)	(0.229)	(0.215)
Georgia	1.657	1.458	1.696	1.772
	(0.177)	(0.092)	(0.170)	(0.133)
Louisiana	1.723	1.106	0.971	0.960
	(0.478)	(0.095)	(0.182)	(0.176)
Mississippi	2.303	2.299	2.085	2.032
	(0.313)	(0.304)	(0.210)	(0.205)
North Carolina	1.539	1.451	0.743	0.687
	(0.130)	(0.126)	(0.064)	(0.059)
South Carolina	2.618	2.556	1.784	1.742
	(0.301)	(0.283)	(0.241)	(0.234)
All States	1.938	1.791	1.755	1.783
	(0.110)	(0.089)	(0.108)	(0.102)
Panel B: White Moves out of Great Plains				
Kansas	0.255	0.220	0.243	0.228
	(0.024)	(0.019)	(0.021)	(0.019)
Nebraska	0.361	0.253	0.265	0.253
	(0.082)	(0.014)	(0.019)	(0.017)
North Dakota	0.464	0.464	0.527	0.531
	(0.036)	(0.036)	(0.046)	(0.046)
Oklahoma	0.453	0.395	0.450	0.427
	(0.036)	(0.029)	(0.040)	(0.038)
South Dakota	0.350	0.339	0.387	0.381
	(0.026)	(0.026)	(0.034)	(0.033)
All States	0.380	0.331	0.374	0.361
	(0.022)	(0.012)	(0.016)	(0.016)

Notes: All columns contain weighted averages of social interaction estimates, $\hat{\Delta}_k$, where the weights are the number of people who move from each state to destination k. Column 1 includes all birth towns and destinations. Column 2 excludes birth towns with 1920 population greater than 20,000 when estimating each $\hat{\Delta}_k$. Column 3 excludes all destination counties which intersect in 2000 with the ten largest non-South CMSAs as of 1950: New York, Chicago, Los Angeles, Philadelphia, Boston, Detroit, Washington D.C., San Francisco, Pittsburgh, and St. Louis, in addition to counties which received fewer than 10 migrants. Column 4 excludes large birth towns and large destinations. Birth town groups are defined by cross validation. Standard errors are in parentheses.
Source: Authors' calculations using Duke SSA/Medicare data

Table 2.6: Average Social Interactions Index Estimates, by Region

	Destination Region			
	Northeast (1)	Midwest (2)	West (3)	South (4)
Panel A: Black Moves out of South				
Alabama	1.237	2.356	0.813	-
	(0.161)	(0.295)	(0.272)	-
Florida	0.978	0.793	0.264	-
	(0.172)	(0.169)	(0.107)	-
Georgia	1.546	2.067	0.410	-
	(0.243)	(0.310)	(0.205)	-
Louisiana	0.282	1.138	2.169	-
	(0.101)	(0.206)	(0.734)	-
Mississippi	0.924	2.662	1.036	-
	(0.105)	(0.396)	(0.130)	-
North Carolina	1.678	0.908	0.185	-
	(0.149)	(0.176)	(0.040)	-
South Carolina	2.907	1.223	0.211	-
	(0.351)	(0.167)	(0.055)	-
All States	1.860	2.259	1.402	-
	(0.120)	(0.195)	(0.345)	-
Panel B: White Moves out of Great Plains				
Kansas	0.079	0.452	0.281	0.051
	(0.019)	(0.095)	(0.031)	(0.006)
Nebraska	0.080	0.439	0.420	0.063
	(0.014)	(0.096)	(0.109)	(0.009)
North Dakota	0.107	0.405	0.524	0.047
	(0.027)	(0.057)	(0.046)	(0.009)
Oklahoma	0.051	0.390	0.542	0.074
	(0.007)	(0.091)	(0.047)	(0.007)
South Dakota	0.061	0.485	0.381	0.058
	(0.013)	(0.069)	(0.034)	(0.011)
All States	0.073	0.434	0.442	0.062
	(0.007)	(0.039)	(0.029)	(0.004)

Notes: All columns contain weighted averages of destination level social interactions index estimates, $\hat{\Delta}_k$, where the weights are the number of people who move from each state to destination k. See footnote 36 for region definitions. We do not estimate social interactions for blacks who move to the South. Birth town groups are defined by cross validation. Standard errors are in parentheses.
Source: Authors' calculations using Duke SSA/Medicare data

Table 2.7: Social Interactions Index Estimates and Destination County Characteristics, Black Moves out of South

Dependent Variable: Destination Level Social Interaction Estimate	(1)	(2)	(3)
Manufacturing employment share, 1910	1.651***	1.139***	1.076***
	(0.396)	(0.353)	(0.360)
Manufacturing employment share by small destination indicator		1.122**	1.145**
		(0.564)	(0.546)
Small destination indicator		0.129	0.108
		(0.132)	(0.127)
Direct railroad connection from birth state	0.033	-0.005	-0.058
	(0.119)	(0.117)	(0.133)
One-stop railroad connection from birth state	0.065	0.044	-0.007
	(0.084)	(0.079)	(0.083)
Log distance from birth state	-0.405***	-0.339***	-0.395***
	(0.062)	(0.063)	(0.066)
Log number of migrants from birth state	0.316***	0.351***	0.353***
	(0.035)	(0.036)	(0.035)
Log population, 1900	-0.131***	-0.110***	-0.110***
	(0.037)	(0.035)	(0.037)
Percent African-American, 1900	-2.142***	-1.655***	-1.703***
	(0.336)	(0.327)	(0.328)
Birth state fixed effects			x
Observations	1,469	1,469	1,469
Clusters	371	371	371
R-squared	0.178	0.199	0.209

Notes: The dependent variable is the social interaction estimate for each destination county by birth state pair. The sample contains only counties which received at least 10 migrants. Birth town groups are defined by cross validation. Standard errors, clustered by destination county, are in parentheses. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

Source: Authors' calculations using Duke SSA/Medicare data, Haines and ICPSR (2010) data, and Black et al. (2015*a*) data

Table 2.8: Social Interactions Index Estimates and Birth County Characteristics, Black Moves out of South

Dependent Variable: Birth County Level Social Interaction Estimate	(1)	(2)
Share with income less than $2,000 (1950)	3.302**	4.853***
	(1.482)	(1.815)
Percent rural, 1950	-2.812	-3.441*
	(1.795)	(1.925)
Rosenwald exposure	-0.768	-0.867
	(0.683)	(0.762)
Railroad exposure	-0.083	-0.048
	(0.471)	(0.474)
Percent African-American	0.600	0.284
	(0.836)	(1.115)
Log number of migrants	0.508***	0.527**
	(0.165)	(0.239)
Birth state fixed effects		x
Observations	551	551
R-squared	0.084	0.095

Notes: The dependent variable is the birth county level social interaction estimate. Railroad exposure is the share of migrants in a county which lived along a railroad. Rosenwald exposure is the average Rosenwald coverage experienced over ages 7-13. Robust standard errors are in parentheses. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

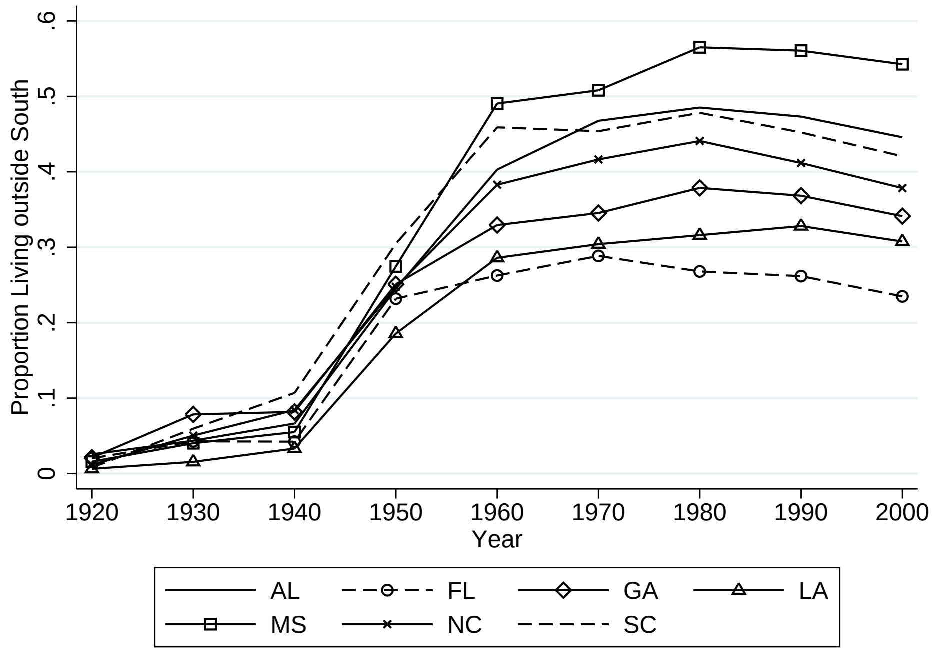
Sources: Authors' calculations using Duke SSA/Medicare data, Haines and ICPSR (2010) data, Aaronson and Mazumder (2011) data, and Black et al. (2015*a*) data

Table 2.9: Estimated Share of Migrants That Chose Their Destination Because of Social Interactions

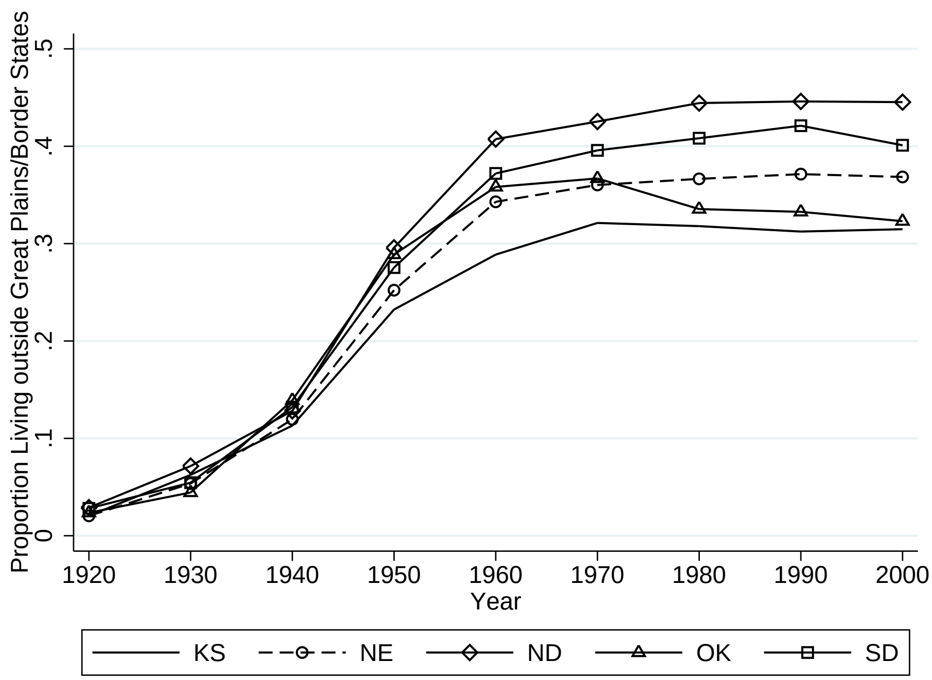
	Destination Region				
	All	Northeast	Midwest	West	South
Birth State	(1)	(2)	(3)	(4)	(5)
Panel A: Black Moves out of South					
Alabama	0.486	0.382	0.541	0.289	-
	(0.026)	(0.031)	(0.031)	(0.069)	-
Florida	0.289	0.328	0.284	0.117	-
	(0.030)	(0.039)	(0.043)	(0.042)	-
Georgia	0.453	0.436	0.508	0.170	-
	(0.026)	(0.039)	(0.038)	(0.070)	-
Louisiana	0.463	0.123	0.363	0.520	-
	(0.069)	(0.039)	(0.042)	(0.084)	-
Mississippi	0.535	0.316	0.571	0.341	-
	(0.034)	(0.025)	(0.036)	(0.028)	-
North Carolina	0.435	0.456	0.312	0.085	-
	(0.021)	(0.022)	(0.042)	(0.017)	-
South Carolina	0.567	0.592	0.379	0.095	-
	(0.028)	(0.029)	(0.032)	(0.023)	-
All States	0.492	0.482	0.530	0.412	-
	(0.014)	(0.016)	(0.022)	(0.060)	-
Panel B: White Moves out of Great Plains					
Kansas	0.113	0.038	0.184	0.123	0.025
	(0.009)	(0.009)	(0.032)	(0.012)	(0.003)
Nebraska	0.153	0.039	0.180	0.174	0.031
	(0.029)	(0.007)	(0.032)	(0.037)	(0.004)
North Dakota	0.188	0.051	0.168	0.208	0.023
	(0.012)	(0.012)	(0.020)	(0.015)	(0.004)
Oklahoma	0.185	0.025	0.163	0.213	0.036
	(0.012)	(0.003)	(0.032)	(0.015)	(0.003)
South Dakota	0.149	0.030	0.195	0.160	0.028
	(0.010)	(0.006)	(0.022)	(0.012)	(0.005)
All States	0.160	0.035	0.178	0.181	0.030
	(0.008)	(0.003)	(0.013)	(0.010)	(0.002)

Notes: Table contains estimates and standard errors of $\chi = \Delta/(2+\Delta)$, the share of migrants which chose their destination because of social interactions, based on weighted average estimates from column 3 of table 2.3 and columns 1-4 of table 2.6. Standard errors, estimated using the Delta method, are in parentheses.
Source: Authors' calculations using Duke SSA/Medicare data

Figure 2.1: Proportion Living Outside Home Region, 1916-1936 Birth Cohorts, by Birth State and Year



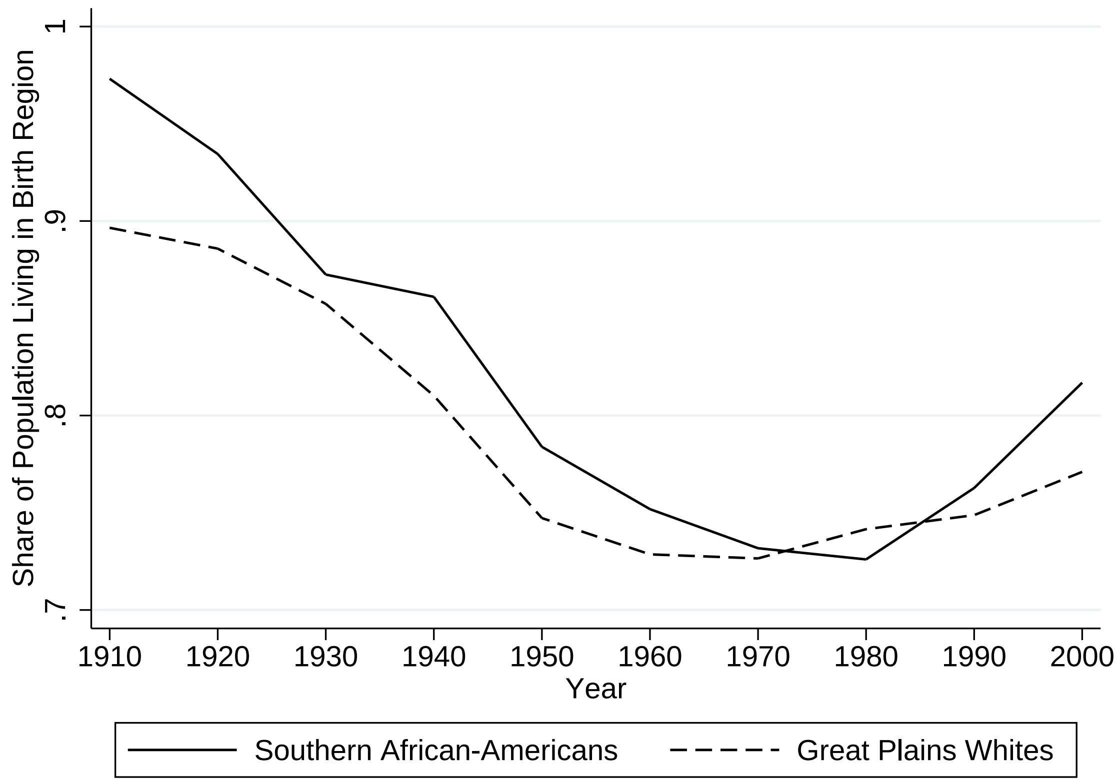
(a) Southern Blacks



(b) Great Plains Whites

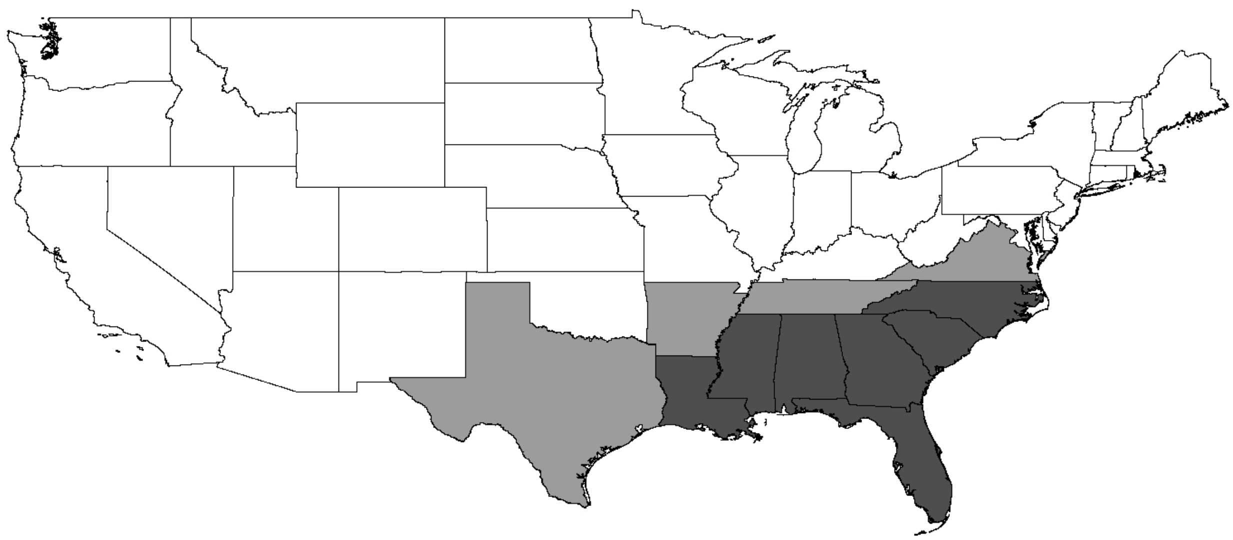
Notes: See notes to figure 2.3 for home region definitions.
Source: Authors' calculations using Ruggles et al. (2010) data

Figure 2.2: Trajectory of Migrations out of South and Great Plains

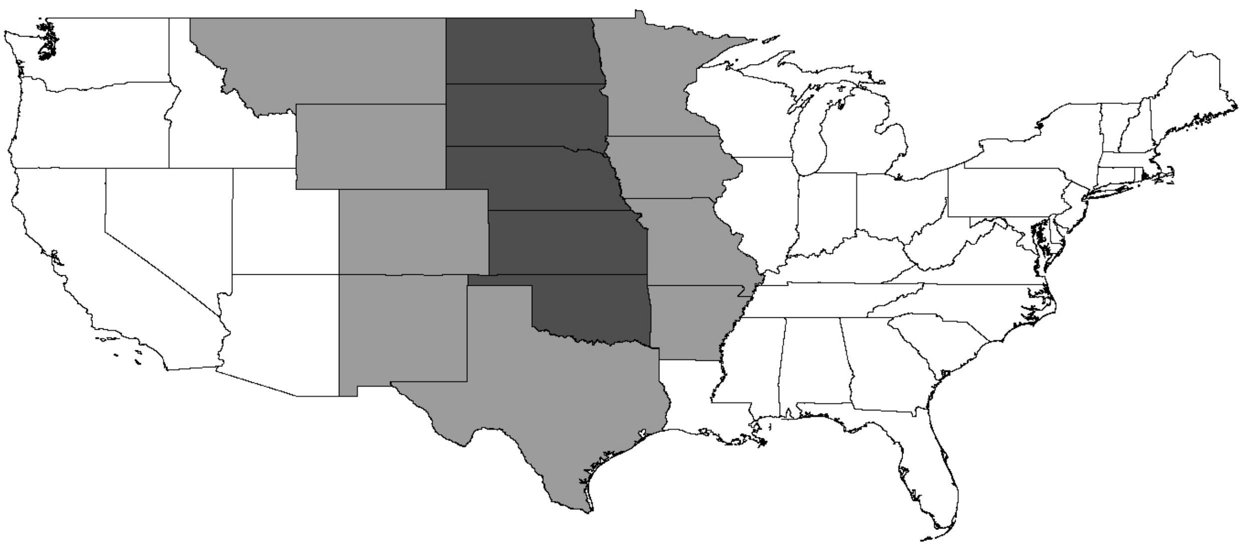


Notes: The solid line shows the proportion of blacks from the seven Southern birth states we analyze (dark grey states in Figure 2.3a) living in the South (light and dark grey states) at the time of Census enumeration. The dashed line shows the proportion of whites from the Great Plains states living in the Great Plains or Border States.
Source: Authors' calculations using Ruggles et al. (2010) data

Figure 2.3: Geographic Coverage



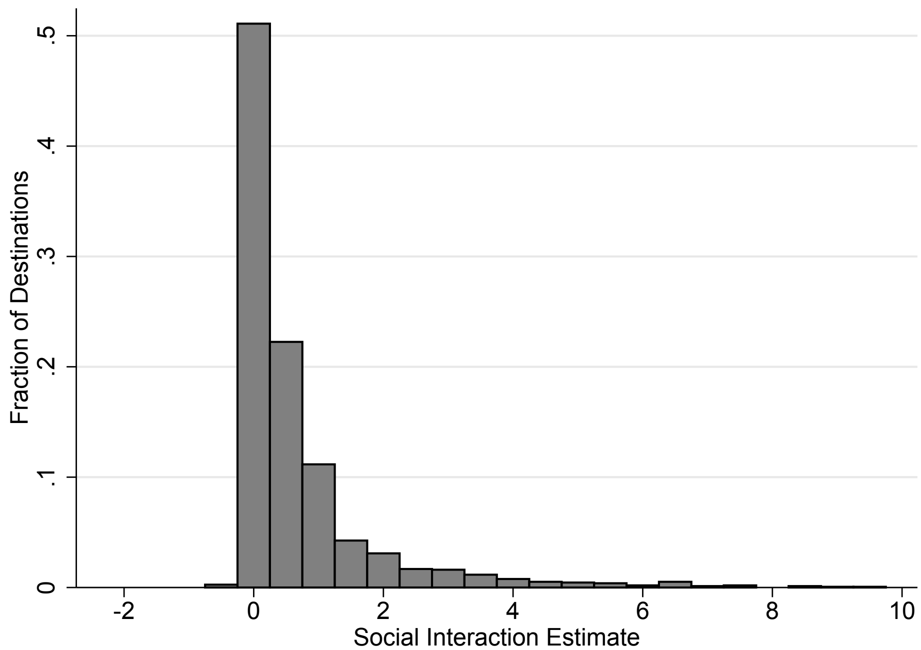
(a) South



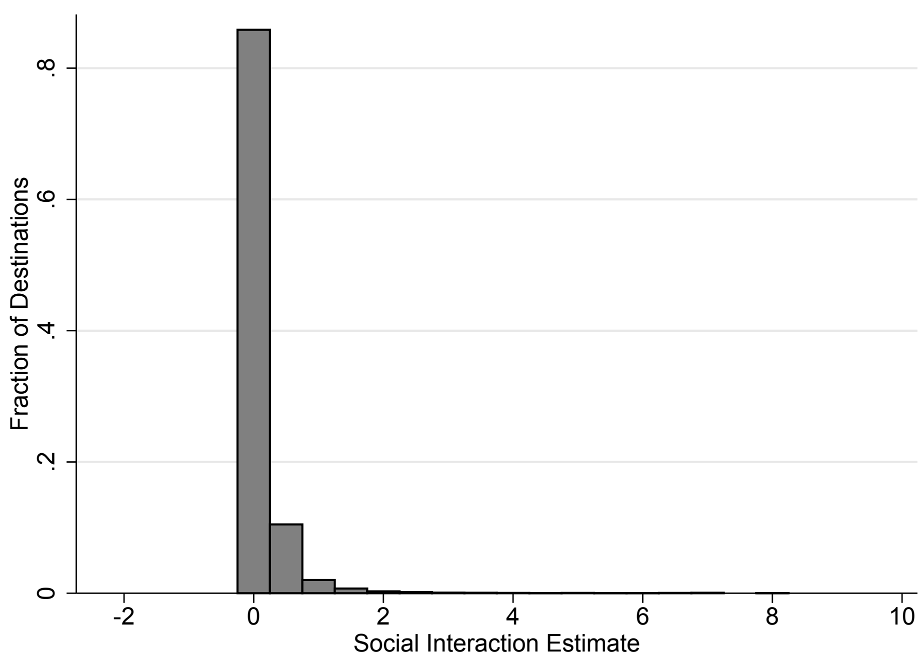
(b) Great Plains

Notes: For the South, our sample includes migrants born in the seven states in dark grey (Alabama, Georgia, Florida, Louisiana, Mississippi, North Carolina, South Carolina). A migrant is someone who at old age lives outside of the Confederacy, which includes the dark and light grey states. For the Great Plains, our sample includes migrants born in the five states in dark grey (Kansas, Nebraska, North Dakota, Oklahoma, South Dakota). A migrant is someone who at old age lives outside of the Great Plains states and the surrounding border area.

Figure 2.4: Distribution of Destination Level Social Interaction Estimates



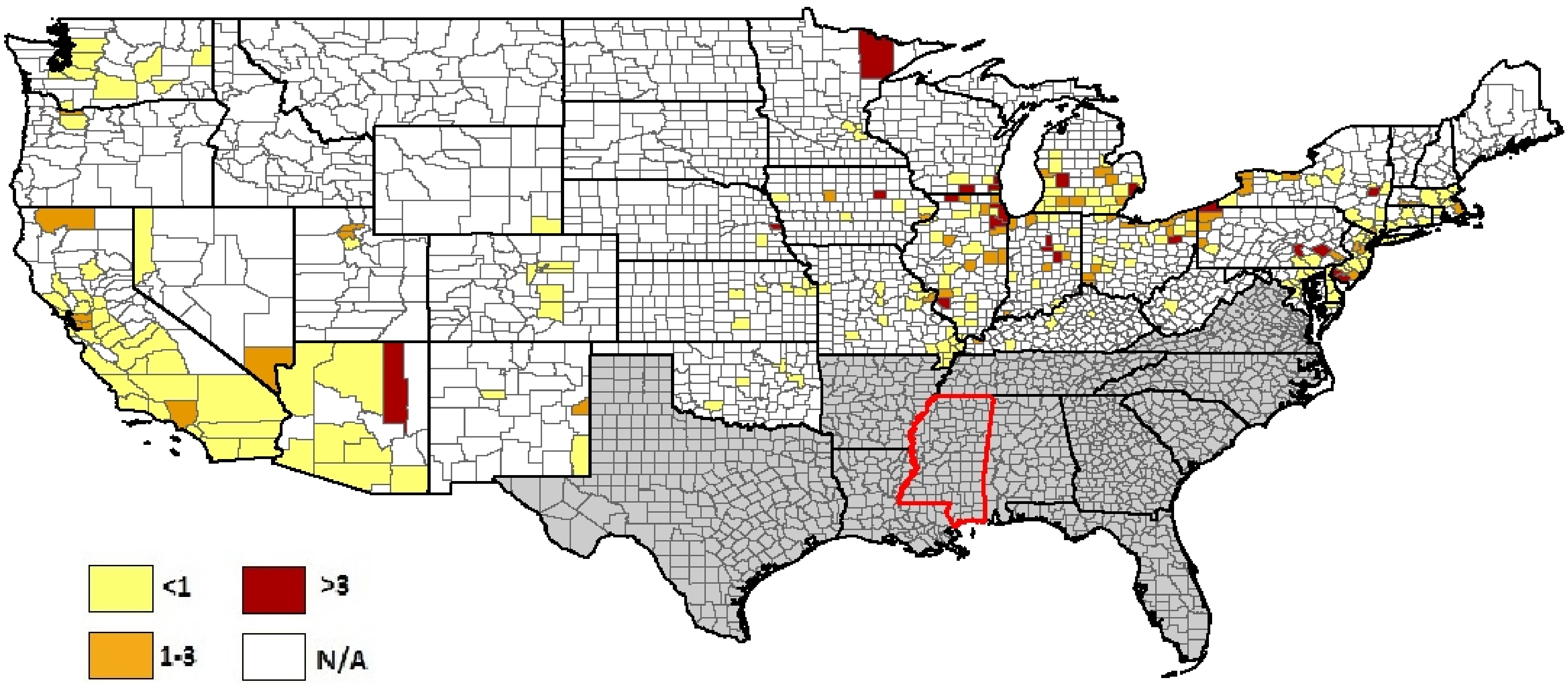
(a) Black Moves out of South



(b) White Moves out of Great Plains

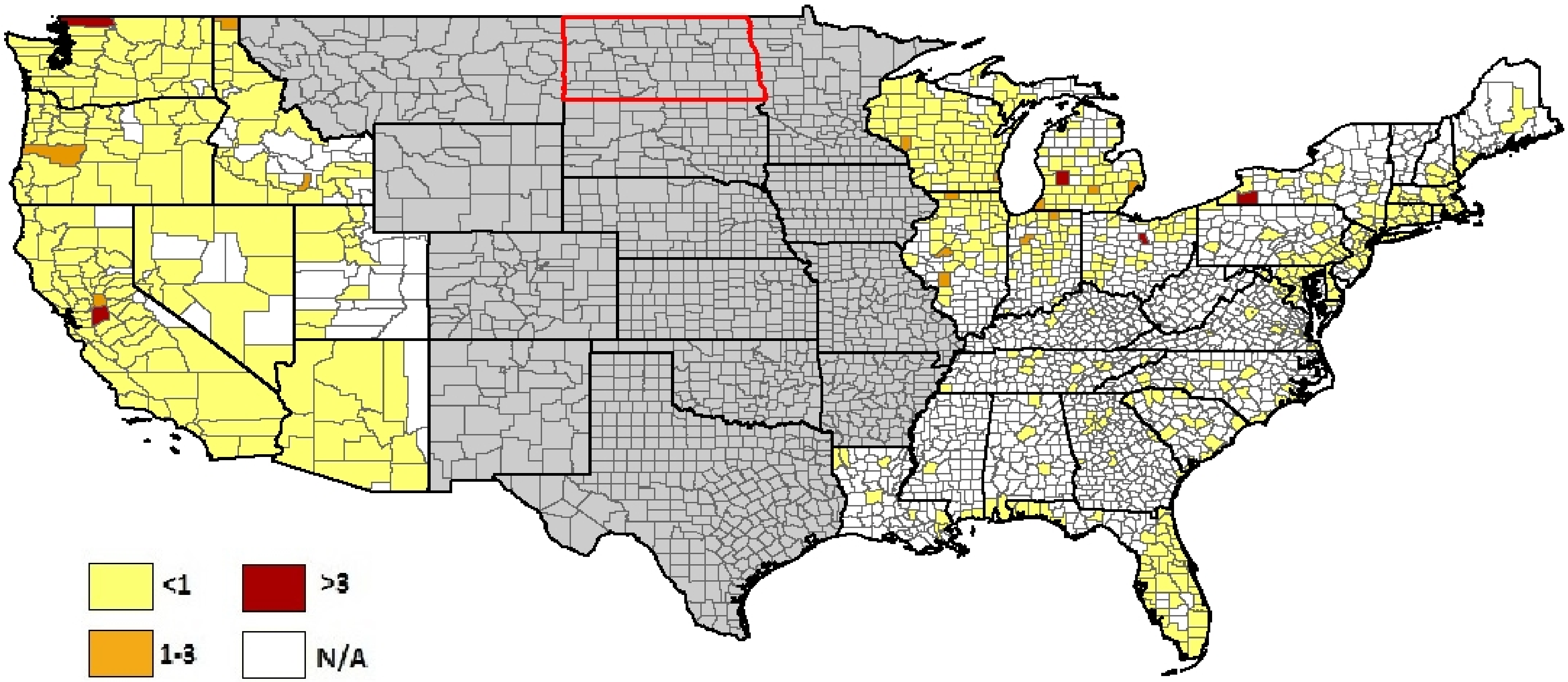
Notes: Bin width is 1/2. Birth town groups are defined by cross validation. Panel (a) omits the estimate $\hat{\Delta}_k = 11.4$ from Mississippi to Racine County, WI, $\hat{\Delta}_k = 15.2$ from South Carolina to Rensselaer County, NY, and $\hat{\Delta}_k = 18.1$ from Florida to St. Joseph County, IN.
Source: Authors' calculations using Duke SSA/Medicare data

Figure 2.5: Spatial Distribution of Destination Level Social Interaction Estimates, Mississippi-born Blacks



Notes: Figure displays destination level social interaction estimates, $\hat{\Delta}_k$, across U.S. counties for Mississippi-born black migrants. The South is shaded in grey, with Mississippi outlined in red. Destinations to which less than 10 migrants moved are in white. Among all African-American estimates, $\hat{\Delta}_k = 3$ corresponds to the 95th percentile, while $\hat{\Delta}_k = 1$ corresponds to the 81st percentile.
Source: Authors' calculations using Duke SSA/Medicare data

Figure 2.6: Spatial Distribution of Destination Level Social Interaction Estimates, North Dakota-born Whites



Notes: See note to Figure 2.5. Among all Great Plains white estimates, $\hat{\Delta}_k = 3$ is greater than the 99th percentile, while $\hat{\Delta}_k = 1$ corresponds to the 98th percentile.
Source: Authors' calculations using Duke SSA/Medicare data

CHAPTER III

The Effect of Social Connectedness on Crime: Evidence from the Great Migration

3.1 Introduction

For almost 200 years, the enormous variance of crime rates across space has intrigued social scientists and policy makers (Guerry, 1833; Quetelet, 1835; Weisburd, Bruinsma and Bernasco, 2009). Standard covariates explain a modest amount of cross-city variation in crime, which suggests a potential role for social influences. One possible explanation is peer effects, whereby an individual is more likely to commit crime if his peers commit crime (e.g., Case and Katz, 1991; Glaeser, Sacerdote and Scheinkman, 1996; Damm and Dustmann, 2014). A non-rival explanation is that cities differ in the degree of social connectedness, or the strength of relationships between individuals. Despite vast academic and public interest in the related concept of social capital, concerns about reverse causality and omitted variables seriously limit existing evidence on the effect of social connectedness on crime.

This paper uses a new source of variation in social connectedness to estimate its effect on crime. Social interactions in the migration of millions of African Americans out of the U.S. South from 1915-1970 generated plausibly exogenous variation across destinations in the concentration of migrants that came from the same birth town. For example, consider Beloit, Wisconsin and Middletown, Ohio, two cities similar along many dimensions, including the total number of Southern black migrants that moved there. Around 18 percent of Beloit's black migrants came from Pontotoc, Mississippi, while less than five percent of Middletown's migrants came from any single town. Historical accounts trace the sizable migration from Pontotoc to Beloit to a single influential migrant getting a job in 1914 at a manufacturer in search of workers. Furthermore, qualitative evidence suggests that Southern birth town networks translated into strong community ties in the North. Guided by a simple economic model, we proxy for social connectedness using a Herfindahl-Hirschman Index of birth town to destination city population flows for individuals born from 1916-1936 who we observe in the Duke SSA/Medicare dataset.

Economic theory does not make an unambiguous prediction about whether social connectedness will increase or decrease crime. Social connectedness could increase crime by reinforcing unproductive norms or providing trust that facilitates criminal activity, as with the Ku Klux Klan, Mafia, or gangs (Fukuyama, 2000; Putnam, 2000). Alternatively, social connectedness could decrease crime by increasing the probability that criminals are identified and punished (Becker, 1968) or by facilitating the development of cognitive and non-cognitive skills during childhood (Heckman, Stixrud and Urzua, 2006).

We estimate regressions that relate cross-city differences in crime from 1960-2009 to cross-city differences in social connectedness. We control for the number of Southern black migrants that live in each city to adjust for differences in the overall attractiveness of cities to black migrants, and we control for a rich set of demographic and economic variables, plus state-by-year fixed effects, that might influence crime. We measure city-level crime data using FBI Uniform Crime Reports, which are widely available starting in 1960.

We find that social connectedness leads to sizable reductions in crime rates. At the mean, a one standard deviation increase in social connectedness leads to a precisely estimated 14.1 percent decrease in murder, the best measured crime in FBI data. Our estimates imply that replacing Middletown's social connectedness with that of Beloit would decrease murders by 25.4 percent, robberies by 35.2 percent, and motor vehicle thefts by 22.9 percent. By comparison, the estimates in Chalfin and McCrary (2015) imply that a similar decrease in murders would require a 38 percent increase in the number of police officers. The elasticity of crime with respect to social connectedness ranges from -0.05 to -0.25 across the seven commonly studied index crimes of murder, rape, robbery, assault, burglary, larceny, and motor vehicle theft, and is statistically distinguishable from zero for every crime besides larceny. As predicted by our economic model, the effect of social connectedness on city-level crime rates is stronger in cities with a higher African American population share. Social connectedness reduces crimes that are more and less likely to have witnesses, which suggests that an increased probability of detection is not the only operative mechanism.

The substantial reductions in crime due to social connectedness are not permanent. We estimate significant negative effects of social connectedness in each decade from 1960-1999, and much smaller and insignificant effects from 2000-2009. The attenuated effects from 2000-2009 appear to reflect a decline in the effective strength of social connectedness, as Southern black migrants aged and eventually died. From 1980-1989, social connectedness reduces murders attributed to African American adults and especially African American youth, who belong to the generation of the migrants' children and grandchildren. Social connectedness also reduces murders attributed to non-blacks, consistent with an important role of peer effects.

Several pieces of evidence support the validity of our empirical strategy. Historical accounts point to the importance of migrants who were well connected in their birth town and who worked for an employer in search of labor in establishing concentrated migration flows from Southern birth towns to Northern cities (Scott, 1920; Bell, 1933; Gottlieb, 1987; Grossman, 1989). Many of the initial location decisions were made in the 1910's, over 40 years before we estimate effects on crime. Consistent with the dominant role of idiosyncratic factors, social connectedness is not correlated with crime rates from 1911-1916 or in a consistent manner with economic or demographic covariates from 1960-2000.[1] One potential threat to our empirical strategy is that migrants from the same birth town tended to move to cities with low unobserved determinants of crime and these unobserved determinants of crime persisted over time. We provide evidence that this threat

[1]The one exception is that social connectedness is positively correlated with the share of a destination's work force employed in manufacturing, a relatively attractive sector for African American migrants (Stuart and Taylor, 2017). We control for a city's manufacturing employment share in our regressions.

is unimportant by showing that the estimated effect of social connectedness on crime after 1965 is very similar when we control for the 1960-1964 crime rate. We also show that our results are robust to controlling for the share of migrants in each destination that moved there because of social interactions, a variable we obtain by estimating a novel structural model of social interactions in location decisions. Consequently, our estimates likely reflect the effect of social connectedness per se, as opposed to unobserved characteristics of certain migrants.

This paper contributes most directly to the literature studying how characteristics of social networks affect crime. Arguably the best available evidence comes from Sampson, Raudenbush and Earls (1997), who examine the neighborhood-level relationship in Chicago between crime and proxies for collective efficacy, defined as "social cohesion among neighbors combined with their willingness to intervene on behalf of the common good" (p. 918). Despite extremely rich data, their proxies could be correlated with unobserved determinants of crime.[2] We contribute by providing a new source of plausibly exogenous variation in social connectedness and new evidence. We also use a simple economic model to highlight the important interaction between social connectedness and peer effects.

We also contribute to the literature in economics studying the impact of social capital and trust on various outcomes, including growth and development (Knack and Keefer, 1997; Miguel, Gertler and Levine, 2005), government efficiency and public good provision (La Porta et al., 1997; Alesina, Baqir and Easterly, 1999, 2000), financial development (Guiso, Sapienza and Zingales, 2004), and the repayment of microfinance loans (Karlan, 2005, 2007; Cassar, Crowley and Wydick, 2007; Feigenberg, Field and Pande, 2013). We differ from most of this work by focusing on social connectedness, as opposed to social capital or trust, and by using plausibly exogenous cross-city variation in social connectedness.[3] Several papers also examine the determinants of social capital and trust (Alesina and Ferrara, 2000; Glaeser et al., 2000; Glaeser, Laibson and Sacerdote,

[2]Sampson, Raudenbush and Earls (1997) acknowledge that "causal effects were not proven" (p. 923) in their study.

[3]Social connectedness is a broader concept than social capital, trust, or collective efficacy. For example, social connectedness might reduce crime by increasing the probability that criminals are identified, and this behavior typically is not included in definitions of social capital, trust, or collective efficacy. At the same time, our measure might capture social capital that was transported from South to North.

2002; Karlan et al., 2009; Sapienza, Toldra-Simats and Zingales, 2013). Our results point to the importance of social interactions in location decisions in generating social connectedness.

More broadly, there is enormous interest in the causes and consequences of criminal activity and incarceration in U.S. cities, especially for African Americans (Freeman, 1999; Neal and Rick, 2014; Evans, Garthwaite and Moore, 2016), and this paper demonstrates the importance of social connectedness among African Americans in reducing crime. We also add to the literature on the consequences of the Great Migration for migrants and cities (e.g., Scroggs, 1917; Smith and Welch, 1989; Carrington, Detragiache and Vishwanath, 1996; Collins, 1997; Boustan, 2009, 2010; Hornbeck and Naidu, 2014; Black et al., 2015*b*). This paper draws on Stuart and Taylor (2017), which examines the importance of social interactions in location decisions for African American migrants in more detail.

3.2 Historical Background on the Great Migration

The Great Migration saw nearly six million African Americans leave the South from 1910 to 1970 (Census, 1979).[4] Although migration was concentrated in certain destinations, like Chicago, Detroit, and New York, other cities also experienced dramatic changes. For example, Chicago's black population share increased from two to 32 percent from 1910-1970, while Racine, Wisconsin experienced an increase from 0.3 to 10.5 percent (Gibson and Jung, 2005). Migration out of the South increased from 1910-1930, slowed during the Great Depression, and then resumed forcefully from 1940 to the 1970's.

Several factors contributed to the exodus of African Americans from the South. World War I, which simultaneously increased labor demand among Northern manufacturers and decreased labor supply from European immigrants, helped spark the Great Migration, although many underlying causes existed long before the war (Scroggs, 1917; Scott, 1920; Gottlieb, 1987; Marks, 1989; Jackson, 1991; Collins, 1997; Gregory, 2005). Underlying causes included a less developed Southern economy, the decline in agricultural labor demand due to the boll weevil's destruction

[4]Parts of this section come from Stuart and Taylor (2017).

of crops (Scott, 1920; Marks, 1989, 1991; Lange, Olmstead and Rhode, 2009), widespread labor market discrimination (Marks, 1991), and racial violence and unequal treatment under Jim Crow laws (Tolnay and Beck, 1991).

Migrants tended to follow paths established by railroad lines: Mississippi-born migrants predominantly moved to Illinois and other Midwestern states, and South Carolina-born migrants predominantly moved to New York and Pennsylvania (Scott, 1920; Carrington, Detragiache and Vishwanath, 1996; Collins, 1997; Boustan, 2010; Black et al., 2015*b*). Labor agents, offering paid transportation, employment, and housing, directed some of the earliest migrants, but their role diminished sharply after the 1920's, and most individuals paid for the relatively expensive train fares themselves (Gottlieb, 1987; Grossman, 1989).[5] African-American newspapers from the largest destinations circulated throughout the South, providing information on life in the North (Gottlieb, 1987; Grossman, 1989).[6] Blacks attempting to leave the South sometimes faced violence (Scott, 1920; Henri, 1975).

Historical accounts and recent quantitative work indicate that social interactions strongly affected location decisions during the Great Migration. Initial migrants, most of whom moved in the 1910's, chose their destination primarily in response to economic opportunity. Migrants who worked for an employer in search of labor and were well connected in their birth town linked friends, family, and acquaintances to jobs and shelter in the North, sometimes leading to persistent migration flows from birth town to destination city (Rubin, 1960; Gottlieb, 1987; Stuart and Taylor, 2017). Stuart and Taylor (2017) show that birth town-level social interactions strongly influenced the location decisions of African American migrants from the South. These social interactions mirror vertical migration patterns established by railroad lines and were stronger in destinations with more manufacturing employment, a particularly attractive sector for black workers during this time.

The experience of John McCord captures many important features of early black migrants'

[5]In 1918, train fare from New Orleans to Chicago cost $22 per person, when Southern farmers' daily wages typically were less than $1 and wages at Southern factories were less than $2.50 (Henri, 1975).

[6]The *Chicago Defender*, perhaps the most prominent African-American newspaper of the time, was read in 1,542 Southern towns and cities in 1919 (Grossman, 1989).

location decision.[7] Born in Pontotoc, Mississippi, nineteen-year-old McCord traveled in search of higher wages in 1912 to Savannah, Illinois, where a fellow Pontotoc-native connected him with a job. McCord moved to Beloit, Wisconsin in 1914 after hearing of employment opportunities and quickly began working as a janitor at the manufacturer Fairbanks Morse and Company. After two years in Beloit, McCord spoke to his manager about returning home for a vacation. The manager asked McCord to recruit workers during the trip. McCord returned with 18 unmarried men, all of whom were soon hired. Thus began a persistent flow of African Americans from Pontotoc to Beloit: among individuals born from 1916-1936, 14 percent of migrants from Pontotoc lived in Beloit's county at old age (Stuart and Taylor, 2017).[8]

Qualitative evidence documents the importance of social ties among African Americans from the same birth town for life in the North. For example, roughly 1,000 of Erie, Pennsylvania's 11,600 African American residents once lived in Laurel, Alabama, and almost half had family connections to Laurel, leading an Erie resident to say, "I'm surrounded by so many Laurelites here, it's like a second home" (Associated Press, 1983). Nearly forty percent of the migrants in Decatur, Illinois came from Brownsville, Tennessee, and Brownsville high school reunions took place in Decatur from the 1980's to 2000's (Laury, 1986; Smith, 2006).[9] As described by a Brownsville native, "Decatur's a little Brownsville, really" (Laury, 1986).

3.3 A Simple Model of Crime and Social Connectedness

This section describes a simple model of crime and social connectedness. Social connectedness, or the strength of relationships between individuals, could reduce crime through multiple channels, including by increasing the probability that criminals are identified and punished or by facilitating the development of human capital during childhood. We use the model to derive an empirical measure of social connectedness, and we show how the effect of social connectedness on crime depends on peer effects.

[7]The following paragraph draws on Bell (1933). See also Knowles (2010).
[8]This is 68 times larger than the percent of migrants from Mississippi that lived in Beloit's county at old age.
[9]The 40 percent figure comes from the Duke SSA/Medicare dataset, described below.

3.3.1 Individual Crime Rates

We focus on a single city and characterize individuals by their age and social ties. For simplicity, we consider a static model in which each younger individual makes a single decision about whether to commit crime, while older individuals do not commit crime. Each individual belongs to one of three groups: blacks with ties to the South ($\tau_i = s$), blacks without ties to the South ($\tau_i = n$), and all others ($\tau_i = w$). Older individuals have a tie to the South if they were born there. Younger individuals have a tie to the South if at least one of their parents, who are older individuals, was born in the South. We index younger individuals by i and older individuals by o.

For a younger individual who is black with ties to the South, we model the probability of committing crime as

$$\mathbb{E}[C_i|\tau_i = s, j_i = j] = \alpha^s + \beta^s\,\mathbb{E}[C_{-i}] + \sum_o \gamma^s_{i,o,j}, \tag{3.1}$$

where $C_i = 1$ if person i commits crime and $C_i = 0$ otherwise, and j_i denotes the birth town of i's parents. Equation (3.1) is a linear approximation to the optimal crime rule from a utility-maximizing model in which the relative payoff of committing crime depends on three factors. First, α^s, which is common to all individuals of type s, captures all non-social determinants of crime (e.g., due to police or employment opportunities). Second, an individual's decision to commit crime depends on the expected crime rate among his peers, $\mathbb{E}[C_{-i}]$. Finally, the effect of social connectedness is $\sum_o \gamma^s_{i,o,j}$, where $\gamma^s_{i,o,j}$ is the influence of older individual o on younger individual i. This reduced-form representation captures several possible channels through which social connectedness might affect crime. For example, older individuals might reduce crime among younger individuals by increasing the probability a criminal is identified and punished (Becker, 1968) or by increasing younger individuals' stock of cognitive and non-cognitive skills, which boost earnings in the non-crime labor market (Heckman, Stixrud and Urzua, 2006). Alternatively, social connectedness could increase crime by reinforcing unproductive norms or providing trust that facilitates criminal activity, as with the Ku Klux Klan, Mafia, or gangs (Fukuyama, 2000; Putnam, 2000). Ethnographic work describing African American families and kinship networks suggests crime-reducing effects of social connectedness (Stack, 1970).

Motivated by the qualitative evidence described in Section 3.2, we model social connectedness as a function of whether the parents of individual i share a birth town with individual o. In particular, $\gamma^s_{i,o,j} = \gamma^s_H$ if the individuals share a birth town connection, $j_i = j_o$, and $\gamma^s_{i,o,j} = \gamma^s_L$ otherwise. We assume that younger blacks with ties to the South are only influenced by older blacks with ties to the South, so that $\gamma^s_{i,o,j} = 0$ if $\tau_i \neq \tau_o$. Given these assumptions, the effect of social connectedness on person i is a weighted average of the high connectedness effect (γ^s_H) and the low connectedness effect (γ^s_L),

$$\sum_o \gamma^s_{i,o,j} = \frac{N^s_{j,0}}{N^s_0}\gamma^s_H + \left(1 - \frac{N^s_{j,0}}{N^s_0}\right)\gamma^s_L, \tag{3.2}$$

where $N^s_{j,0}$ is the number of older individuals of type s from birth town j, and $N^s_0 = \sum_j N^s_{j,0}$ is the total number of older individuals in the city. Because social interactions depend on birth town connections, the older generation's migration decisions lead to differences in expected crime rates for younger individuals with ties to different birth towns.

The Herfindahl-Hirschman Index emerges as a natural way to measure social connectedness in this model. In particular, the probability that a randomly chosen African American with ties to the South commits crime is

$$\mathbb{E}[C_i|\tau_i = s] = \alpha^s + \beta^s\,\mathbb{E}[C_{-i}] + \gamma^s_L + (\gamma^s_H - \gamma^s_L)\mathrm{HHI}^s, \tag{3.3}$$

where $\mathrm{HHI}^s \equiv \sum_j (N^s_{j,0}/N^s_0)^2$ is the Herfindahl-Hirschman Index of birth town to destination city population flows for African Americans with ties to the South.[10] The direct effect of social connectedness on the type s crime rate is $\gamma^s_H - \gamma^s_L$. One reasonable case is $\gamma^s_H < \gamma^s_L < 0$, so that older individuals discourage younger individuals from committing crime, and the effect is stronger

[10]In deriving equation (3.3), we assume that each Southern birth town accounts for the same share of individuals in the younger and older generations, so that $N^s_{j,0}/N^s_0 = N^s_{j,1}/N^s_1 \forall j$, where $N^s_{j,1}$ is the number of younger individuals of type s with a connection to birth town j, and $N^s_1 = \sum_j N^s_{j,1}$ is the total number of younger individuals.

among individuals who share a birth town connection. Expressions analogous to equation (3.3) exist for African American youth without ties to the South ($\tau_i = n$) and non-black youth ($\tau_i = w$).

3.3.2 City-Level Crime Rates

We next consider the equilibrium of this model, in which peer effects can accentuate or attenuate the effect of social connectedness on crime. We use HHI to measure social connectedness and allow peer effects to differ by the type of peer, leading to the following equilibrium,

$$\bar{C}^s = F^s(\alpha^s, \text{HHI}^s, \bar{C}^s, \bar{C}^n, \bar{C}^w) \tag{3.4}$$

$$\bar{C}^n = F^n(\alpha^n, \text{HHI}^n, \bar{C}^s, \bar{C}^n, \bar{C}^w) \tag{3.5}$$

$$\bar{C}^w = F^w(\alpha^w, \text{HHI}^w, \bar{C}^s, \bar{C}^n, \bar{C}^w), \tag{3.6}$$

where $\bar{C}^\tau$ is the crime rate among younger individuals of type τ, and F^τ characterizes the equilibrium crime rate responses. The equilibrium crime rate vector $(\bar{C}^s, \bar{C}^n, \bar{C}^w)$ is a fixed point of equations (3.4)-(3.6).

We are interested in the effect of social connectedness among African Americans with ties to the South, HHI^s, on equilibrium crime rates. Equations (3.4)-(3.6) imply that

$$\frac{d\bar{C}^s}{d\text{HHI}^s} = \frac{\partial F^s}{\partial \text{HHI}^s}\left(\frac{(1-J_{22})(1-J_{33}) - J_{23}J_{32}}{\det(I-J)}\right) \equiv \frac{\partial F^s}{\partial \text{HHI}^s} m^s \tag{3.7}$$

$$\frac{d\bar{C}^n}{d\text{HHI}^s} = \frac{\partial F^s}{\partial \text{HHI}^s}\left(\frac{J_{23}J_{31} + J_{21}(1-J_{33})}{\det(I-J)}\right) \equiv \frac{\partial F^s}{\partial \text{HHI}^s} m^n \tag{3.8}$$

$$\frac{d\bar{C}^w}{d\text{HHI}^s} = \frac{\partial F^s}{\partial \text{HHI}^s}\left(\frac{J_{21}J_{32} + J_{31}(1-J_{22})}{\det(I-J)}\right) \equiv \frac{\partial F^s}{\partial \text{HHI}^s} m^w, \tag{3.9}$$

where I is the 3×3 identity matrix and J, a sub-matrix of the Jacobian of equations (3.4)-(3.6), captures the role of peer effects.[11] Equations (3.7)-(3.9) depend on the direct effect of HHI^s on

[11] In particular,

$$J \equiv \begin{bmatrix} \partial F^s/\partial \bar{C}^s & \partial F^s/\partial \bar{C}^n & \partial F^s/\partial \bar{C}^w \\ \partial F^n/\partial \bar{C}^s & \partial F^n/\partial \bar{C}^n & \partial F^n/\partial \bar{C}^w \\ \partial F^w/\partial \bar{C}^s & \partial F^w/\partial \bar{C}^n & \partial F^w/\partial \bar{C}^w \end{bmatrix},$$

and J_{ab} is the (a,b) element of J. m^s is the $(1,1)$ element of $(I-J)^{-1}$, m^n is the $(2,1)$ element, and m^w is the crime among blacks with ties to the South, $\partial F^s/\partial \text{HHI}^s$, times a peer effect multiplier, given by m^s, m^n, and m^w. We assume the equilibrium is stable, which essentially means that peer effects are not too large.[12] For example, if $J_{11} \equiv \partial F^s/\partial \bar{C}^s \geq 1$, and there are no cross-group peer effects, then a small increase in the crime rate among individuals of type s leads to an equilibrium where all individuals of type s commit crime. In contrast, a small change in any group's crime rate does not lead to a corner solution in a stable equilibrium.

Our first result is that if social connectedness reduces the crime rate of African Americans with ties to the South, then social connectedness reduces the crime rate of all groups, as long as the equilibrium is stable and peer effects (i.e., elements of J) are non-negative.

Proposition 1. *$d\bar{C}^s/dHHI^s \leq 0, d\bar{C}^n/dHHI^s \leq 0$, and $d\bar{C}^w/dHHI^s \leq 0$ if $\partial F^s/\partial HHI^s < 0$, the equilibrium is stable, and peer effects are non-negative.*

In a stable equilibrium with non-negative peer effects, the crime-reducing effect of social connectedness among Southern blacks is not counteracted by higher crime rates among other groups. Hence, equilibrium crime rates of all groups weakly decrease in Southern African American HHI. With negative cross-group peer effects, the reduction in crime rates among Southern blacks could lead to higher crime by other groups. Proposition 1 is not surprising, and we provide a proof in Appendix C.1.

Because of data limitations, most of our empirical analysis examines the city-level crime rate, $\bar{C}$, which is a weighted average of the three group-specific crime rates,

$$\bar{C} = P^b[P^{s|b}\bar{C}^s + (1 - P^{s|b})\bar{C}^n] + (1 - P^b)\bar{C}^w, \tag{3.10}$$

where P^b is the black population share and $P^{s|b}$ is the share of the black population with ties to the South. Proposition 1 provides sufficient, but not necessary, conditions to ensure that Southern black HHI decreases the city-level crime rate, $\bar{C}$, when the direct effect is negative. There exist

$(3, 1)$ element.

[12]The technical assumption underlying stability is that the spectral radius of J is less than one. This condition is analogous to the requirement in linear-in-means models that the slope coefficient on the endogenous peer effect is less than one in absolute value (e.g., Manski, 1993).

situations in which cross-group peer effects are negative, but an increase in HHI^s still decreases in the city-level crime rate.

Our second result is that the effect of Southern black social connectedness on the city-level crime rate decreases (or, increases in magnitude) with the black population share for certain peer effect parametrizations.

Proposition 2. *$d\bar{C}/dHHI^s$ decreases with P^b if $\partial F^s/\partial HHI^s < 0$, the equilibrium is stable, and cross-group peer effects are non-negative and sufficiently small.*

We assume that the effect of HHI^s on each group's crime rate does not depend on the black population share, yielding[13]

$$\frac{d^2\bar{C}}{d\text{HHI}^s dP^b} = P^{s|b}\frac{d\bar{C}^s}{d\text{HHI}^s} + (1-P^{s|b})\frac{d\bar{C}^n}{d\text{HHI}^s} - \frac{d\bar{C}^w}{d\text{HHI}^s}. \tag{3.11}$$

Two jointly sufficient conditions for Proposition 2 are (a): $d\bar{C}^s/d\text{HHI}^s < d\bar{C}^w/d\text{HHI}^s$ and (b): $d\bar{C}^n/d\text{HHI}^s \leq d\bar{C}^w/d\text{HHI}^s$. If Southern black social connectedness leads to greater crime reductions among both groups of African Americans, relative to non-blacks, then the total effect will be larger in magnitude in cities with a higher black population share. In this case, Proposition 2 occurs mechanically. The nature of peer effects determines whether conditions (a) and (b) are satisfied, and we provide precise conditions in Appendix C.1.

As a simple example, suppose there are no cross-group peer effects between blacks and non-blacks ($J_{13} = J_{23} = J_{31} = J_{32} = 0$). In this case, an increase in HHI^s does not affect the crime rate among non-blacks, so condition (a) holds. Condition (b) requires that an increase in HHI^s must not increase crime among blacks without ties to the South, which will be true if peer effects between the two groups of African Americans are non-negative. As shown in Appendix C.1, the formal conditions in this example are a stable equilibrium and $J_{21} \geq 0$.

[13] It is not clear whether we would expect, say, $d\bar{C}^s/d\text{HHI}^s$ to be more or less negative in cities with higher P^b. The effect could decrease in magnitude if the higher black population share diluted existing community ties, or the effect could increase in magnitude if the higher black population share reinforced community ties. The former case makes Proposition 2 less likely to hold, while the latter case makes it more likely.

In sum, we expect that higher social connectedness among African Americans with ties to the South will reduce the city-level crime rate (Proposition 1). We also expect that the effect will be stronger in cities with a higher black population share (Proposition 2). Furthermore, the effect of social connectedness among African Americans with ties to the South on the city-level crime rate depends critically on the nature of a peer effects, an issue we examine more fully in Section 3.6 after presenting our baseline results.

3.4 Data and Empirical Strategy

3.4.1 Data on Crime, Social Connectedness, and Control Variables

To estimate the effect of social connectedness on crime, we use three different data sets. We measure annual city-level crime counts using FBI Uniform Crime Report (UCR) data for 1960-2009, available from ICPSR. UCR data contain voluntary monthly reports on the number offenses reported to police, which we aggregate to the city-year level.[14] We focus on the seven commonly studied index crimes: murder and non-negligent manslaughter ("murder"), forcible rape ("rape"), robbery, assault, burglary, larceny, and motor vehicle theft. Murder is the best measured crime, and robbery and motor vehicle theft are also relatively well-measured (Blumstein, 2000; Tibbetts, 2012). Because missing observations are indistinguishable from true zeros, we drop any city-year in which any of the three property crimes (burglary, larceny, and motor vehicle theft) equal zero. We also use annual population estimates from the Census Bureau in the UCR data.

The Duke SSA/Medicare dataset provides the birth town-to-destination city population flows that underlie our measure of social connectedness. The data contain sex, race, date of birth, date of death (if deceased), and the ZIP code of residence at old age (death or 2001, whichever is earlier) for over 70 million individuals who received Medicare Part B from 1976-2001. In addition, the data include a 12-character string with self-reported birth town information, which is matched to

[14] We use Federal Information Processing System (FIPS) place definitions of cities. We follow Chalfin and McCrary (2015) in decreasing the number of murders for year 2001 in New York City by 2,753, the number of victims of the September 11 terrorist attack.

places, as described in Black et al. (2015*b*). We focus on individuals born from 1916-1936 in the former Confederate states, which we refer to as the South.[15] We restrict our main analysis sample to cities that received at least 25 Southern-born African American migrants in the Duke dataset to improve the reliability of our estimates.

Census city data books provide numerous city-level covariates for 1960, 1970, 1980, 1990, and 2000. These data are only available for cities with at least 25,000 residents in 1960, 1980, and 1990, and we apply the same restriction for 1970 and 2000. We limit our sample to cities in the Northeast, Midwest, and West Census regions to focus on the cross-region moves that characterize the Great Migration. Our main analysis sample excludes the 14 cities with 1980 population greater than 500,000, as we found considerable measurement error in murder counts for these cities.[16] Appendix Tables C.1 and C.2 provide summary statistics.

3.4.2 Estimating the Effect of Social Connectedness on Crime

Our main estimating equation is

$$Y_{k,t} = \exp[\ln(\text{HHI}_k)\delta + \ln(N_k)\theta + X'_{k,t}\beta] + \epsilon_{k,t}, \tag{3.12}$$

where $Y_{k,t}$ is the number of crimes in city k in year t. The key variable of interest is our proxy for social connectedness among African Americans with ties to the South, $\text{HHI}_k = \sum_j (N_{j,k}/N_k)^2$, where $N_{j,k}$ is the number of migrants from birth town j that live in destination city k, and $N_k \equiv \sum_j N_{j,k}$ is the total number of migrants. A Herfindahl-Hirschman Index is a natural way to measure social connectedness, as shown in Section 3.3, and approximately equals the probability that two randomly chosen migrants living in city k share a birth town.[17] $X_{k,t}$ is a vector of covari-

[15] Coverage rates decline considerably for earlier and later cohorts (Black et al., 2015*b*; Stuart and Taylor, 2017).

[16] In particular, we constructed annual murder counts using the FBI UCR data, which are not broken down by age, race, or sex, and the FBI Age-Sex-Race (ASR) data, which are. Both data sets should yield the same number of murders in a city, but substantial discrepancies exist in the largest cities (see Appendix Figure C.1). We do not know why the murder counts differ between these data sets.

[17] The probability that two randomly chosen migrants living in city k share a birth town is

$$\mathbb{P}[j_i = j_{i'}] = \sum_j \mathbb{P}[j_i = j_{i'} | j_{i'} = j]\, \mathbb{P}[j_i = j] = \sum_j \frac{N_{j,k} - 1}{N_k - 1} \frac{N_{j,k}}{N_k} \approx \text{HHI}_k.$$

ates, including log population and other variables described below, and $\epsilon_{k,t}$ captures unobserved determinants of crime.[18] We use an exponential function in equation (3.12) because there are no murders for many city-year observations (Appendix Table C.1). We cluster standard errors at the city level to allow for arbitrary autocorrelation in the unobserved determinants of crime.[19]

The key parameter of interest is δ, which we interpret as the elasticity of the crime *rate* with respect to HHI_k, our proxy of social connectedness, because we control for log population. If social connectedness reduces the city-level crime rate, as predicted by Proposition 1, then $\delta < 0$.

We estimate δ using cross-city variation in social connectedness, conditional on the total number of migrants and other covariates. To identify δ, we make the following conditional independence assumption,

$$\epsilon_{k,t} \perp\!\!\!\perp \text{HHI}_k | (N_k, X_{k,t}). \tag{3.13}$$

Condition (3.13) states that, conditional on the number of migrants living in city k and the vector of control variables, social connectedness is independent of unobserved determinants of crime from 1960-2009. This condition allows the total number of migrants, N_k, to depend arbitrarily on unobserved determinants of crime, $\epsilon_{k,t}$.[20]

We include several control variables in $X_{k,t}$ that bolster the credibility of condition (3.13). State-by-year fixed effects flexibly account for determinants of crime that vary over time at the state-level, due to changes in economic conditions, police enforcement, government spending, and other factors. Demographic covariates include log population, percent black, percent female, percent age 5-17, percent age 18-64, percent age 65 and older, percent at least 25 years old with a high school degree, percent at least 25 years old with a college degree, and log city area. Economic

[18]Because equation (3.12) includes $\ln(\text{HHI}_k)$, $\ln(N_k)$, and log population, our estimate of δ would be identical if we used city population as the denominator of HHI_k.

[19]Equation (3.12) emerges from a Poisson model, but consistent estimation of (δ, θ, β) does not require any restriction on the conditional variance of the error term (e.g., Wooldridge, 2002).

[20]Condition (3.13) does not guarantee identification of the other parameters in equation (3.12) besides δ. For example, identification of θ requires exogenous variation in the total number of migrants in each city. Boustan (2010) provides one possible strategy for such an approach, but we do not pursue that here.

covariates include log median family income, unemployment rate, labor force participation rate, and manufacturing employment share.[21] We observe log population in every year and, with a few exceptions, we observe the remaining demographic and economic covariates every ten years from 1960-2000.[22] In explaining crime in year t, we only use covariates corresponding to the decade in which t lies. We allow coefficients for all covariates besides log population to vary across decades to account for possible changes in the importance of economic and demographic covariates.

Several pieces of evidence support the validity of condition (3.13). First, variation in social connectedness stems from location decisions made 50 years before we estimate effects on crime. As described in Section 3.2, initial migrants in the 1910's chose their destination in response to economic opportunity, and idiosyncratic factors, like a migrant's ability to persuade friends and family to join them, strongly influenced whether other migrants followed.[23]

Table 3.1 shows that social connectedness is not correlated with homicide rates from 1911-1914. In particular, we regress $\ln(\text{HHI}_k)$ on $\ln(N_k)$ and log homicide rates from 1911-1914, which we observe from historical mortality statistics published for cities with at least 100,000 residents in 1920 (Census, 1922). We find no significant relationship between social connectedness and early century crime rates. This conclusion holds when we use inverse probability weights to make this sample of cities more comparable to our main analysis sample on observed characteristics.[24] These results partially dismiss the possibility that social connectedness is correlated with extremely persistent unobserved determinants of crime, which would threaten our empirical strategy.

[21] Stuart and Taylor (2017) find that the manufacturing employment share predicts the strength of social interactions in location decisions among Southern black migrants, which leads to higher social connectedness.

[22] The exceptions are percent female (not observed in 1960), percent at least 25 years old with a high school degree and a college degree (not observed in 2000), log median family income (not observed in 2000), and manufacturing share (not observed in 2000). For decades in which a covariate is not available, we use the adjacent decade.

[23] For example, Scott (1920) writes, "The tendency was to continue along the first definite path. Each member of the vanguard controlled a small group of friends at home, if only the members of his immediate family. Letters sent back, representing that section of the North and giving directions concerning the route best known, easily influenced the next groups to join their friends rather than explore new fields. In fact, it is evident throughout the movement that the most congested points in the North when the migration reached its height, were those favorite cities to which the first group had gone" (p. 69).

[24] We do not adjust the standard errors in columns 3-4 for the use of inverse probability weights. As a result, the p-values for these columns are likely too small, which further reinforces our finding of no significant relationship. Appendix Table C.3 compares the observed characteristics of cities for which we do and do not observe 1911-1914 mortality rates.

If anything, limitations in the data used to construct HHI_k could lead us to understate any negative effect of social connectedness on crime. We construct HHI_k using migrants' location at old age, measured at some point from 1976-2001. As a result, migration after 1960, when we first measure crime, could influence HHI_k and the estimated effect on crime, δ. If migrants with a higher concentration of friends and family nearby were less likely to out-migrate in response to higher crime shocks, $\epsilon_{k,t}$, then HHI_k would be larger in cities with greater unobserved determinants of crime. This would bias our estimate of δ upwards, making it more difficult to conclude that social connectedness reduces crime. Reassuringly, Table 3.2 reveals very low migration rates during this period among African Americans who were born from 1916-1936 in the South and living in the North. Around 90 percent of individuals stayed in the same county for the five-year periods from 1955-1960, 1965-1970, 1975-1980, 1985-1990, and 1995-2000.[25] This table suggests that our inability to construct HHI_k using migrants' location before 1960 is relatively unimportant.

Table 3.3 provides additional indirect evidence in support of condition (3.13) by showing that social connectedness is not systematically correlated with most demographic or economic covariates. The lack of systematic correlations with observed variables suggests that social connectedness is not correlated with unobserved determinants of crime, $\epsilon_{k,t}$. We regress log HHI on various covariates for the 228 cities observed in every decade from 1960 to 2000. To facilitate comparisons, we normalize all variables, separately for each decade, to have mean zero and standard deviation one. Only the log number of migrants and the manufacturing employment share are consistently correlated with log HHI. The negative correlation between log HHI and the log number of migrants arises because a large number of migrants necessarily came from many sending towns, due to the small size of Southern towns relative to Northern cities. The positive correlation between log HHI and the manufacturing employment share arises because social interactions in location decisions guided migrants to destinations with ample manufacturing employment, which was especially attractive to African American workers (Stuart and Taylor, 2017). The bottom panel reports p-values from tests that demographic or economic covariates (besides the manufacturing employment share)

[25] Available data do not allow us to examine whether out-migration rates vary with the concentration of friends and family living nearby, which is the type of behavior that would affect HHI_k.

are unrelated to log HHI. We fail to reject this null hypothesis at standard significance levels from 1960-1980, providing support for condition (3.13). There is a significant relationship between social connectedness and covariates in 1990 and 2000, but this does not necessarily provide evidence against condition (3.13) because social connectedness might have affected these later outcomes.[26] Appendix Table C.4 shows results when adding a number of covariates measured among African-Americans.

Figure 3.1 further describes the cross-city variation in social connectedness by plotting log HHI and the log number of Southern black migrants. Our regressions identify the effect of social connectedness on crime with variation in HHI conditional on the number of migrants in a city (and other covariates), which is variation in the vertical dimension of Figure 3.1. Except for cities with at least 500,000 residents in 1980, there is considerable variation in log HHI conditional on the log number of migrants. Figure 3.2 shows that social connectedness stems largely from the location decisions of a single sending town. Sixty-seven percent of the variation in log HHI is explained by the leading term of log HHI, which equals the log squared share of migrants from the top sending town. This finding reinforces the importance of idiosyncratic features of migrants and birth towns in generating variation in social connectedness.[27]

[26]The significant relationship between social connectedness and demographic covariates in 1990 and 2000 is driven by a negative relationship between social connectedness and the percent of the population age 0-4. Social connectedness could lower birth rates by increasing the opportunity cost of having children (by increasing human capital). The significant relationship between social connectedness and economic covariates in 1990 is driven by a negative relationship between social connectedness and log median income. Social connectedness and log median income are not significantly correlated in other decades.

[27]Appendix Table C.5 displays the relationship between log HHI and estimates of social capital, based mainly on 1990 county-level data, from Rupasingha, Goetz and Freshwater (2006). Raw correlations between log HHI and various measures of social capital are positive, but small and indistinguishable from zero. After controlling for the log number of migrants and state fixed effects, these correlations shrink even further. The social capital estimates of Rupasingha, Goetz and Freshwater (2006) depend on the density of membership organizations, voter turnout for presidential elections, response rates for the decennial Census, and the number of non-profit organizations. The weak correlation between log HHI and the county-level social capital estimates is not particularly surprising, given the different time periods involved and, more importantly, the fact that these social capital estimates do not isolate social ties among African Americans.

3.5 The Effect of Social Connectedness on Crime

3.5.1 Effects on City-Level Crime Rates

Motivated by the model in Section 3.3, we estimate the effect of social connectedness on city-level crime rates (Proposition 1) and whether this effect is stronger in cities with a higher African American population share (Proposition 2).

Table 3.4 shows that social connectedness leads to sizable and statistically significant reductions in murder, rape, robbery, assault, burglary, and motor vehicle theft. The table reports estimates of equation (3.12) for an unbalanced panel of 471 cities.[28] As seen in column 1, our estimated elasticity of the murder rate with respect to HHI is -0.181 (0.034). The estimates for robbery and motor vehicle theft, two other well-measured crimes in the FBI data, are -0.251 (0.035) and -0.163 (0.041). These results are consistent with Proposition 1.

Because social connectedness reduces crimes that are more and less likely to have witnesses, an increased probability of detection likely is not the only operative mechanism. Burglary and motor vehicle theft are less likely to have witnesses than rape, robbery, or assault, yet our estimates are roughly comparable for all of these crimes.[29] As a result, the effect of social connectedness on crime probably stems in part from other mechanisms, such as an improvement in cognitive or non-cognitive skills.

Simple examples help illustrate the sizable effects of social connectedness on crime. First, consider Middletown, Ohio and Beloit, Wisconsin. These cities are similar in their total number of Southern black migrants, 1980 population, and 1980 black population share, but Beloit's HHI is over four times as large as in Middletown (0.057 versus 0.014).[30] The estimates in Table 3.4 imply that replacing Middletown's HHI with that of Beloit would decrease murders by 25.4 percent, robberies by 35.2 percent, and motor vehicle thefts by 22.9 percent. By comparison, the estimates

[28] Appendix Table C.6 displays results for all covariates in the regressions.

[29] Unlike larceny or motor vehicle theft, a robbery features the use of force or threat of force. Consequently, robberies are witnessed by at least one individual (the victim).

[30] For Middletown and Beloit, the number of Southern black migrants is 376 and 407; the 1980 population is 35,207 and 43,719; and the 1980 percent black is 11.3 and 12.0.

in Chalfin and McCrary (2015) imply that a similar decrease in murders would require a 38 percent increase in the number of police officers.[31] The effect of social connectedness is even larger in other examples. HHI in Decatur, Illinois is almost twenty times larger than that of Albany, NY (0.118 versus 0.006).[32] Replacing Albany's HHI with that of Decatur would decrease murders by 53.9 percent, robberies by 74.8 percent, and motor vehicle thefts by 48.6 percent. While these effects are sizable, they are reasonable in light of the tremendous variation in crime rates across cities (Appendix Table C.2).

Table 3.5 demonstrates that our results are robust to various sets of control variables. We focus on the effect of social connectedness on murder, given its importance for welfare and high measurement quality, and we restrict the sample to the 228 cities observed in every decade. Our baseline specification in column 1 yields an estimate of δ of -0.244 (0.041). Estimates are very similar when excluding demographic or economic covariates (columns 2-3) and somewhat attenuated when excluding both sets of covariates or replacing state-year fixed effects with region-year fixed effects (columns 4-5). The estimate is even larger in magnitude when not controlling for the log number of migrants and is very similar when using ten indicator variables to control flexibly for the number of migrants (columns 6-7).[33] Controlling for log HHI and the log number of Southern white migrants and foreign immigrants has little impact on the estimate (column 8).[34] Results are similar when we control for the share of migrants that chose their destination because of social interactions (column 9); this variable controls for unobserved characteristics of migrants that could confound our results, as detailed below.

Table 3.6 provides some evidence that the effect of social connectedness on crime is stronger in cities with a higher African American population share. We estimate equation (3.12) separately for each tercile of cities' 1960 African American population share. Across increasing levels of the

[31] Chalfin and McCrary (2015) estimate an elasticity of murder with respect to police of -0.67, almost four times the size of our estimated elasticity of murder with respect to social connectedness.

[32] For Decatur and Albany, the number of Southern black migrants is 760 and 874; the 1980 population is 94,081 and 101,727; and the 1980 percent black is 14.6 and 15.9.

[33] For identification purposes, we strongly prefer to control for the log number of migrants. We estimate the regression in column 6 to demonstrate that the strong relationship between log HHI and the log number of migrants does not account for the negative coefficient on log HHI.

[34] We use country of birth to construct HHI for immigrants.

black population share, the estimated effect of HHI on murder is -0.017 (0.124), -0.085 (0.052), and -0.213 (0.051). A similar pattern exists for other crimes, including robbery and motor vehicle theft. Point estimates for the highest percent black tercile are negative and statistically significant across all crimes, while point estimates for the lowest percent black tercile are indistinguishable from zero for six out of seven crimes.[35] Moving from the 25th to 75th percentile of HHI (0.008 to 0.028) has essentially no effect on the murder rate in cities in the bottom tercile of black population share. For the middle tercile, increasing HHI across the interquartile range leads to 0.6 fewer murders per 100,000 residents, relative to a base of 5.4 murders per 100,000; the effect is 3.4 fewer murders per 100,000 residents at the highest percent black tercile, relative to a base of 12.8 murders per 100,000. The results in Table 3.6 are consistent with Proposition 2 of the model, which predicts a stronger effect of social connectedness on city-level crime rates in cities with a higher black population share because a higher share of individuals in these cities have social ties to African Americans from the South.

3.5.2 Effects over Time

Table 3.7 shows that the effect of social connectedness on crime is generally smaller in magnitude from 2000-2009 relative to 1960-1999. We estimate equation (3.12) separately for each decade.[36] Focusing on the best measured crimes of murder, robbery, and motor vehicle theft, we see significant negative effects of social connectedness in each decade from 1960-1999, and much smaller and insignificant effects from 2000-2009.

One possible explanation for the attenuated effects from 2000-2009 is a decline in the effective strength of social connectedness over time. Reductions in crime in 1960 were likely driven by individuals who were born around 1940 to mothers born around 1915.[37] More generally, the individuals most affected by social connectedness were likely the children and grandchildren of

[35]However, standard errors for estimates in the lowest percent black tercile are quite large, and we cannot reject equality of coefficients in the low and high terciles for murder ($t = -1.46$) or robbery ($t = -1.42$), but can for motor vehicle theft ($t = -2.35$).

[36]To ensure that our results are not driven by changes in the sample over time, we limit the sample in Table 3.7 to cities that appear in at least five years of every decade.

[37]The highest offending rate for murder is between ages 18-24 (Fox, 2000).

post-war migrants and the grandchildren or great-grandchildren of the earliest group of migrants. As a result, the crime-reducing effect of social connectedness might have declined as the original migrants died. A second possible explanation is that individuals committing crime in the 2000's, when crime rates were relatively low (see Figure 3.3), were inframarginal and not affected by social connectedness.

The attenuated effects from 2000-2009 appear to reflect a decline in the effective strength of social connectedness, as opposed to an interaction between the level of crime and the effect of social connectedness. Figure 3.5 shows that fewer black children had ties to the South from 2000-2009 compared to previous decades. We characterize individuals age 14-17 who are living in the North, Midwest, or West regions as having a tie to the South if they or an adult in their household were born in the South. The share of black children with ties to the South declines from 67 percent in 1980 to 33 percent in 2000 and 20 percent in 2010. We also examine whether the effect of social connectedness from 2000-2009 differs across cities with higher and lower predicted crime rates. In particular, we estimate equation (3.12) using data from 1995-1999 and use the coefficients from this regression to predict cities' crime rates from 2000-2009 based on their economic and demographic covariates.[38] There is little evidence of a negative effect of social connectedness from 2000-2009 even for the cities with higher predicted crime rates (Appendix Table C.7).

Figure 3.4 plots the evolution of crime rates from 1960-2009 for two hypothetical cities with HHI at the 75th and 25th percentiles and average values of other covariates. Crime rates rose much more slowly from 1960-1990 in cities with higher social connectedness. Crime rates for cities with high and low social connectedness converged after 1990. Adding up the effect of social connectedness on crime rates from 1960-2009 implies that the city with HHI at the 75th percentile had 139 fewer murders and 10,822 fewer motor vehicle thefts per 100,000 residents over this period.

[38]We include $\ln(\mathrm{HHI}_k)$ and $\ln(N_k)$ in the 1995-1999 regression, but replace these variables with their mean when constructing predicted crime rates. We also use state-specific linear trends in place of state-by-year fixed effects for these regressions.

3.5.3 Effects by Age and Race of Offender over Time

Table 3.8 shows that social connectedness leads to particularly large reductions in murders committed by black youth. From 1980-1989, the elasticity of murders committed by black youth with respect to social connectedness is -0.761 (0.175), almost four times the size of the elasticity of murders committed by non-black youth.[39] The effect of social connectedness on murders committed by black youth declines over time, consistent with the decline in social ties seen in Figure 3.5. The effect of social connectedness on murders committed by black adults declines more slowly over time, consistent with social connectedness having persistent effects on cohorts. Peer effects provide a natural explanation for the reduction in crime among non-blacks, as described in our model.

3.5.4 Threats to Empirical Strategy and Additional Robustness Checks

A key potential threat to our empirical strategy is that cities with higher social connectedness had lower unobserved determinants of crime, $\epsilon_{k,t}$. For example, if migrants from the same birth town moved to cities with low unobserved determinants of crime, and these unobserved characteristics persisted over time, then our estimate of δ could be biased downwards. We have already presented indirect evidence against this threat by showing that log HHI is not correlated with homicide rates from 1911-1916 (Table 3.1) or most demographic and economic covariates from 1960-2009 (Table 3.3).

To provide more direct evidence against this threat, we estimate the effect of social connectedness on crime for each five-year interval from 1965-2009 while controlling for the log average crime rate from 1960-1964.[40] Figure 3.6 shows that the effect of social connectedness on murder is nearly identical when controlling for the 1960-1964 crime rate. These results directly rule out the possibility that our estimates are driven by a persistent correlation between HHI and unobserved determinants of crime from 1960-forward.[41]

[39]FBI data provide the age, race, and sex of offenders for crimes resulting in arrest starting in 1980.

[40]Controlling for the average log crime rate is unattractive because many cities report zero murders in a given year.

[41]The similarity of the results in Figure 3.6 is not driven by a weak relationship between the log average crime rate

Another possible concern is that HHI reflects unobserved characteristics of migrants who chose the same destination as other individuals from their birth town. Census data show that Southern black migrants living in a state or metropolitan area with a higher share of migrants from their birth state have less education and income (Appendix Table C.8). As a result, migrants who followed their birth town network likely had less education and earnings capacity than other migrants. This negative selection in terms of education and earnings could generate a positive correlation between HHI_k and $\epsilon_{k,t}$, making it more difficult for us to estimate a negative effect of social connectedness on crime. At the same time, migrants who followed their birth town network might have displayed greater cooperation or other "pro-social" behaviors. To address this possibility, we estimate a structural model of social interactions in location decisions. As described in Appendix C.2, the model allows us to estimate the share of migrants in each destination that moved there because of social interactions. When used as a covariate in equation (3.12), this variable proxies for unobserved characteristics of migrants that chose to follow other migrants from their birth town. Column 9 of Table 3.5 shows that the estimated effect of social connectedness on murder barely changes when we control for the share of migrants that chose their destination because of social interactions.[42] Consequently, our results appear to reflect the effect of social connectedness per se, as opposed to unobserved characteristics of certain migrants.

Appendix Table C.9 shows that our results are robust to including the 14 largest cities that are excluded from the main analysis, estimating negative binomial models, dropping outliers of the dependent variable, and measuring HHI using birth county to destination county population flows.[43]

from 1960-1964 and crime rates from 1965-forward.

[42] Results are nearly identical when we use quadratic, cubic, or quartics in this variable.

[43] We prefer equation (3.12) over the negative binomial specification because it requires fewer assumptions to generate consistent estimates of δ (e.g., Wooldridge, 2002).

3.6 Understanding the Role of Peer Effects

We now use the model in Section 3.3 to examine the role of peer effects in mediating the relationship between social connectedness and city-level crime rates. The model connects the total effect of HHI on city-level crime, δ, to the effect of HHI on crime for blacks with ties to the South and peer effects. In particular, equations (3.7)-(3.10) imply that the elasticity of the city-level crime rate with respect to Southern black HHI, δ, can be written

$$\delta = \varepsilon^s r^s \left[P^b(P^{s|b}m^s + (1 - P^{s|b})m^n) + (1 - P^b)m^w\right], \tag{3.14}$$

where $\delta \equiv (d\bar{C}/d\text{HHI}^s)(\text{HHI}^s/\bar{C})$ is the parameter of interest in our regressions, $\varepsilon^s \equiv (\partial F^s/\partial\text{HHI}^s)$ (HHI^s/F^s) captures the direct effect of HHI on the crime rate of blacks with ties to the South, $r^s \equiv \bar{C}^s/\bar{C}$ is the ratio of the crime rate among blacks with ties to the South to the overall crime rate, P^b is the black population share, $P^{s|b}$ is the share of blacks with ties to the South, and m^s, m^n, and m^w are peer effect multipliers defined in equations (3.7)-(3.10).

We use equation (3.14) to examine which direct effect (ε^s) and peer effect (m^s, m^n, m^w) parametrizations are consistent with our central estimate of δ for murder. We set the black population share $P^b = 0.13$ and the share of the black population with ties to the South $P^{s|b} = 0.67$.[44] We do not observe the crime rate among blacks with ties to the South. In the FBI data, half of the murders resulting in arrest are attributed to African Americans. If crime rates are equal among blacks with and without ties to the South, then $r^s = 3.8$.[45]

We make several simplifying assumptions about peer effects. First, we assume that own-group peer effects are equal across all three groups.[46] Second, we assume that cross-group peer effects between non-blacks and both groups of African Americans are equal. Third, we assume that

[44] The black population share in our sample is 0.13 in 1980. As seen in Figure 3.5, the share of African American youth living in the North with ties to the South is 0.67.

[45] If crime rates are equal among blacks with and without ties to the South, then $\bar{C}^s = \bar{C}^b$, where $\bar{C}^b \equiv C^b/N^b$ is the crime rate among all blacks. As a result, $r^s = (C^b/N^b)/(C/N) = (C^b/C)/(N^b/N) = 0.5/0.13$, where C and N are the total number of crimes and individuals. To the extent that blacks with ties to the South commit less crime than blacks without ties to the South, we will overstate r^s and understate the direct effect, ε^s.

[46] We are aware of no evidence suggesting that own-group peer effects differ for black versus non-black youth.

cross-group peer effects are symmetric in terms of elasticities.[47] The first assumption implies that $J_{11} = J_{22} = J_{33}$, and the second implies that $J_{12} = J_{21}$, $J_{13} = J_{23}$, and $J_{31} = J_{32}$. Letting E_{ab} denote the elasticity form of J_{ab}, these three assumptions imply that $E_{11} = E_{22} = E_{33}$, $E_{12} = E_{21}$, and $E_{13} = E_{23} = E_{31} = E_{32}$.

We draw on previous empirical work to guide our parametrization of peer effects. As detailed in Appendix C.3, the literature suggests on-diagonal values of J (own-group peer effects) between 0 and 0.5 and off-diagonal values of J (cross-group peer effects) near zero (Case and Katz, 1991; Glaeser, Sacerdote and Scheinkman, 1996; Ludwig and Kling, 2007; Damm and Dustmann, 2014).[48] We consider on-diagonal values of J of 0, 0.25, and 0.5. We allow for sizable peer effects between African Americans with and without ties to the South, and we parametrize the cross-race effects so that elasticities equal 0 or 0.1. Given values of $(r^s, P^b, P^{s|b}, m^s, m^n, m^w)$ and our estimate of δ, equation (3.14) yields a unique value for ε^s. Equations (3.7)-(3.9) then allow us to to solve for the effect of a change in Southern black HHI on crime rates for each group.[49]

Table 3.9 maps the estimated effect of social connectedness on the city-level murder rate, $\hat{\delta}$, to the effect on murder rates of various groups under different peer effect parametrizations.[50] We consider a one standard deviation increase in HHI, equal to 0.78, which decreases the total murder rate by 14.1 percent according to the estimate in Table 3.4. This implies a decrease in the murder rate of blacks with ties to the South between 42.2 percent, when there are no cross-group peer effects (column 1), and 21.2 percent, when peer effects operate across all groups (column 7). The murder rate of blacks without ties to the South decreases by 0-24.2 percent, while the murder rate of non-blacks decreases by 0-8.0 percent. Depending on the parametrization, up to 82 percent of the effect on blacks with ties to the South is driven by peer effects. The existing evidence on peer

[47]Given the differences in crime rates between blacks and non-blacks, we believe that assuming symmetric cross-group elasticities is more appropriate than assuming symmetric cross-group linear effects (J).

[48]Estimates from previous work are valuable, but are not necessarily comparable to each other or our setting, as they rely on different contexts, identification strategies, data sources, and crime definitions.

[49]In particular: $(d\bar{C}^s/d\text{HHI}^s)(\text{HHI}^s/\bar{C}^s) = \varepsilon^s m^s$, $(d\bar{C}^n/d\text{HHI}^s)(\text{HHI}^s/\bar{C}^n) = \varepsilon^s m^n(\bar{C}^s/\bar{C}^n)$, and $(d\bar{C}^w/d\text{HHI}^s)(\text{HHI}^s/\bar{C}^w) = \varepsilon^s m^w(\bar{C}^s/\bar{C}^w)$. Our assumption that crime rates are equal among blacks with and without ties to the South implies that $\bar{C}^s/\bar{C}^n = 1$. The same assumption, combined with the fact that half of murders are attributed to blacks in the UCR data, implies that $\bar{C}^s/\bar{C}^w = (1 - P^b)/P^b = 6.69$.

[50]Under all peer effect parametrizations in Table 3.9, the equilibrium is stable, and Propositions 1 and 2 are true.

effects suggests placing the most emphasis on columns 3 and 4, which imply that a one standard deviation increase in HHI reduces the murder rate of African Americans with ties to the South by 37.3 and 30.1 percent and reduces the murder rate of African Americans without ties to the South by 9.9 and 8.7 percent.[51] In columns 3 and 4, peer effects account for 30.2 and 32.6 percent of the effect on blacks with ties to the South. Peer effects clearly could play an important role in amplifying the effect of social connectedness on crime.

3.7 Conclusion

This paper estimates the effect of social connectedness on crime across U.S. cities from 1960-2009. We use a new source of variation in social connectedness stemming from social interactions in the migration of millions of African Americans out of the South. A one standard deviation increase in social connectedness leads to a precisely estimated 14 percent decrease in murder. We find that social connectedness also leads to sizable reductions in rapes, robberies, assaults, burglaries, and motor vehicle thefts. As predicted by our economic model, social connectedness leads to greater reductions in the city-level crime rate in cities with a higher African American population share. Social connectedness reduces crimes that are more and less likely to have witnesses, which suggests that an increased probability of detection is not the only mechanism through which social connectedness reduces crime.

Our results highlight the importance of birth town level social ties in reducing violent and property crimes in U.S. cities. In principle, similar social ties among immigrants could reduce crime and generate other desirable outcomes. While the benefits of these social ties must be weighed against any possible offsetting effects (e.g., on assimilation), the characteristics of social networks could prove valuable in achieving difficult economic and social milestones.

In future work, we plan to use our new source of variation in social connectedness to study its effects on a variety of other economic outcomes, such as schooling, employment, marriage, and

[51]The results in Table 3.8, which show significant effects of social connectedness on non-black crime, suggest sizable peer effects between non-blacks and blacks.

fertility. Evidence on these effects is of independent interest and would improve our understanding of the negative effects on crime documented in this paper.

Table 3.1: The Relationship between Social Connectedness and 1911-1916 Homicide Rates

	Dependent variable: Log HHI, Southern black migrants			
	(1)	(2)	(3)	(4)
Log mean homicide rate, 1911-1916	0.010	0.073	0.050	-0.012
	(0.147)	(0.101)	(0.216)	(0.088)
p-value	[0.948]	[0.476]	[0.817]	[0.896]
		(0.055)		(0.043)
Log number, Southern black migrants		x		x
Inverse probability weighted			x	x
R2	0.00	0.43	0.00	0.67
N (cities)	46	46	46	46

Notes: The sample contains cities in the North, Midwest, and West Census regions with at least 100,000 residents in 1920. We exclude homicide rates based on less than five deaths in constructing the mean homicide rate from 1911-1916. In columns 3-4, we use inverse probability weights (IPWs) because the sample of cities for which we observe homicide rates from 1911-1916 differs on various characteristics from our main analysis sample. We construct IPWs using fitted values from a logit model, where the dependent variable is an indicator for a city having homicide rate data for at least one year from 1911-1916, and the explanatory variables are log population, percent black, percent female, percent with a high school degree or more, percent with a college degree or more, log land area, log median family income, unemployment rate, labor force participation rate, and manufacturing employment share, all measured in 1980. Unlike our main analysis sample, we do not restrict the sample to cities with less than 500,000 residents in 1980. Heteroskedastic-robust standard errors in parentheses. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

Sources: Census (1922, p. 64-65) , Duke SSA/Medicare data, Census city data book

Table 3.2: Five-Year Migration Rates, Southern Black Migrants Living Outside of the South

	1955-1960 (1)	1965-1970 (2)	1975-1980 (3)	1985-1990 (4)	1995-2000 (5)
Percent living in same state	93.1	95.5	96.2	96.0	95.9
Same county	86.4	90.4	93.8	77.2	93.8
Same house	33.0	54.0	72.8	77.2	79.1
Different house	53.4	36.4	21.0	-	14.7
Different county	-	4.3	2.4	-	2.1
Unknown	6.7	0.8	-	18.8	-
Percent living in different state	6.9	4.5	3.8	4.0	4.1
Not in South	4.0	2.8	1.4	1.2	1.0
In South	2.9	1.6	2.4	2.9	3.1

Notes: Sample restricted to African Americans who were born in the South from 1916-1936 and were living in the North, Midwest, or West regions five years prior to the census year. For 2000, column 3 equals the percent living in the same PUMA.
Sources: Census IPUMS, 1960-2000

Table 3.3: The Relationship between Social Connectedness and City Covariates, 1960-2000

	Dependent variable: Log HHI, Southern black migrants					
Year covariates are measured:	-	1960	1970	1980	1990	2000
	(1)	(2)	(3)	(4)	(5)	(6)
Log number, Southern	-0.839***	-0.834***	-0.834***	-0.813***	-0.727***	-0.737***
black migrants	(0.040)	(0.066)	(0.072)	(0.078)	(0.082)	(0.072)
Log population		0.013	-0.009	-0.020	-0.065	0.006
		(0.062)	(0.067)	(0.075)	(0.085)	(0.083)
Percent black		0.011	-0.013	-0.005	-0.059	-0.063
		(0.053)	(0.060)	(0.075)	(0.067)	(0.058)
Percent female		0.017	-0.036	-0.004	-0.011	-0.013
		(0.047)	(0.058)	(0.076)	(0.077)	(0.055)
Percent age 5-17		-0.131	0.089	0.161	0.557**	0.324
		(0.151)	(0.204)	(0.242)	(0.248)	(0.292)
Percent age 18-64		-0.117	0.044	0.164	0.586**	0.499
		(0.122)	(0.211)	(0.250)	(0.260)	(0.319)
Percent age 65+		-0.029	0.109	0.236	0.521***	0.393*
		(0.094)	(0.146)	(0.198)	(0.187)	(0.200)
Percent with high school degree		-0.052	-0.065	-0.178*	-0.037	-0.046
		(0.115)	(0.117)	(0.096)	(0.076)	(0.079)
Percent with college degree		0.149**	0.101	0.076	0.118*	0.047
		(0.073)	(0.064)	(0.051)	(0.064)	(0.063)
Log area, square miles		-0.028	0.021	0.022	0.031	-0.021
		(0.049)	(0.060)	(0.065)	(0.073)	(0.078)
Log median family income		-0.032	-0.028	-0.002	-0.238***	-0.070
		(0.085)	(0.084)	(0.089)	(0.089)	(0.065)
Unemployment rate		0.115*	0.147*	0.027	0.001	0.057
		(0.060)	(0.079)	(0.070)	(0.079)	(0.060)
Labor force participation rate		0.024	0.085	0.017	0.106	-0.047
		(0.025)	(0.052)	(0.091)	(0.100)	(0.051)
Manufacturing employment		0.225***	0.166***	0.142**	0.162***	0.190***
share		(0.058)	(0.061)	(0.055)	(0.048)	(0.045)
State fixed effects	x	x	x	x	x	x
Adjusted R2	0.742	0.769	0.763	0.756	0.762	0.769
N (cities)	228	228	228	228	228	228
p-value: Wald test that parameters equal zero						
Demographic covariates		0.239	0.631	0.280	0.022	0.001
Economic covariates		0.121	0.104	0.983	0.012	0.066

Notes: Sample restricted to cities with less than 500,000 residents in 1980. We normalize all variables, separately for each regression, to have mean zero and standard deviation one. For the Wald tests, demographic covariates include log population, percent black, percent female, percent age 5-17, percent age 18-64, percent age 65+, percent with high school degree, percent with college degree, and log area. Economic covariates include log median family income, unemployment rate, and labor force participation rate (but not manufacturing employment share). Heteroskedastic-robust standard errors in parentheses. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

Sources: Duke SSA/Medicare data, Census city data book

Table 3.4: The Effect of Social Connectedness on Crime, 1960-2009

	Dependent variable: Number of offenses reported to police						
	Murder	Rape	Robbery	Assault	Burglary	Larceny	Motor Vehicle Theft
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Log HHI, Southern black migrants	-0.181***	-0.083**	-0.251***	-0.142***	-0.095***	-0.049	-0.163***
	(0.034)	(0.035)	(0.035)	(0.042)	(0.022)	(0.030)	(0.041)
Log number, Southern black migrants	x	x	x	x	x	x	x
Demographic covariates	x	x	x	x	x	x	x
Economic covariates	x	x	x	x	x	x	x
State-year fixed effects	x	x	x	x	x	x	x
Pseudo R2	0.773	0.838	0.931	0.913	0.938	0.926	0.906
N (city-years)	18,854	17,690	18,854	18,854	18,854	18,854	18,854
Cities	471	471	471	471	471	471	471

Notes: Table displays estimates of equation (3.12). Sample restricted to cities with less than 500,000 residents in 1980. Demographic covariates include log population, percent black, percent age 5-17, percent age 18-54, percent 65+, percent female, percent with high school degree, percent with college degree, and log area. Economic covariates include log median family income, unemployment rate, labor force participation rate, and manufacturing employment share. Standard errors, clustered at the city level, are in parentheses. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

Sources: FBI UCR, Duke SSA/Medicare data, Census city data book

Table 3.5: The Effect of Social Connectedness on Murder, 1960-2009, Robustness

	Dependent variable: Number of murders reported to police								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Log HHI, Southern black migrants	-0.244***	-0.269***	-0.228***	-0.163**	-0.157***	-0.342***	-0.222***	-0.234***	-0.278***
	(0.041)	(0.044)	(0.046)	(0.073)	(0.054)	(0.042)	(0.045)	(0.044)	(0.053)
Log number, Southern black migrants	x	x	x	x	x			x	x
Demographic covariates	x		x		x	x	x	x	x
Economic covariates	x	x			x	x	x	x	x
State-year fixed effects	x	x	x	x		x	x	x	x
Region-year fixed effects					x				
Indicators for number of Southern black migrants							x		
Log HHI, Southern white migrants								x	
Log number, Southern white migrants								x	
Log HHI, immigrants								x	
Log number, immigrants								x	
Share of Southern black migrants influenced by social interactions									x
Pseudo R2	0.805	0.796	0.801	0.764	0.787	0.803	0.805	0.805	0.805
N (city-years)	11,284	11,284	11,284	11,284	11,284	11,284	11,284	11,284	11,284
Cities	228	228	228	228	228	228	228	228	228

Notes: Table displays estimates of equation (3.12). Sample restricted to cities with less than 500,000 residents in 1980 that also are observed in every decade from 1960-2000. Demographic covariates include log population, percent black, percent age 5-17, 18-64, and 65+, percent female, percent of population at least 25 years old with a high school degree, percent of population at least 25 years old with a college degree, and log of area in square miles. Economic covariates include log median family income, unemployment rate, labor force participation rate, and manufacturing employment share. Indicators for the number of Southern black migrants correspond to deciles. Column 9 includes an estimate of the share of migrants that chose their destination because of social interactions. We estimate this variable using a structural model of social interactions in location decisions, as described in the text. Standard errors, clustered at the city level, are in parentheses. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

Sources: FBI UCR, Duke SSA/Medicare data, Census city data book

Table 3.6: The Effect of Social Connectedness on Crime, 1960-2009, by Percent Black Tercile

	Dependent variable: Number of offenses reported to police						
	Murder (1)	Rape (2)	Robbery (3)	Assault (4)	Burglary (5)	Larceny (6)	Motor Vehicle Theft (7)
Coefficient on Log HHI, Southern Black Migrants by Percent Black Tercile							
Low	-0.017	-0.118	-0.062	-0.184	-0.067	-0.154*	0.072
	(0.124)	(0.157)	(0.136)	(0.120)	(0.083)	(0.092)	(0.150)
Medium	-0.085	0.053	-0.091	-0.051	-0.043	-0.006	-0.056
	(0.052)	(0.067)	(0.072)	(0.067)	(0.043)	(0.047)	(0.071)
High	-0.213***	-0.195***	-0.264***	-0.280***	-0.117***	-0.147**	-0.304***
	(0.051)	(0.066)	(0.040)	(0.073)	(0.032)	(0.057)	(0.056)

Notes: Table displays estimates of equation (3.12). Sample restricted to cities with less than 500,000 residents in 1980. Regressions include the same covariates used in Table 3.4. Percent black is measured in 1960, and the tercile cutoffs are 0.022 and 0.075. Standard errors, clustered at the city level, are in parentheses. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$
Sources: FBI UCR, Duke SSA/Medicare data, Census city data book

Table 3.7: The Effect of Social Connectedness on Crime, 1960-2009, by Decade

	Dependent variable: Number of offenses reported to police						
	Murder	Rape	Robbery	Assault	Burglary	Larceny	Motor Vehicle Theft
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Coefficient on Log HHI, Southern Black Migrants by Decade							
1960-69	-0.121**	-0.313***	-0.368***	-0.265***	-0.145***	-0.087	-0.198**
	(0.062)	(0.112)	(0.082)	(0.098)	(0.054)	(0.064)	(0.078)
1970-79	-0.273***	-0.220***	-0.327***	-0.179**	-0.133***	-0.033	-0.219***
	(0.055)	(0.046)	(0.057)	(0.082)	(0.031)	(0.045)	(0.067)
1980-89	-0.313***	-0.181***	-0.374***	-0.099	-0.174***	-0.089	-0.307***
	(0.050)	(0.057)	(0.059)	(0.075)	(0.033)	(0.059)	(0.074)
1990-99	-0.285***	-0.068	-0.300***	-0.150***	-0.116***	-0.064	-0.277***
	(0.080)	(0.064)	(0.058)	(0.054)	(0.040)	(0.046)	(0.076)
2000-09	-0.059	0.127**	-0.089	-0.129**	-0.039	-0.033	-0.038
	(0.062)	(0.061)	(0.058)	(0.059)	(0.043)	(0.041)	(0.067)

Notes: Table displays estimates of equation (3.12). Sample contains 240 cities that have less than 500,000 residents in 1980 and appear in at least five years of every decade from 1960-2009. Regressions include the same covariates used in Table 3.4. Standard errors, clustered at the city level, are in parentheses. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

Sources: FBI UCR, Duke SSA/Medicare data, Census city data book

Table 3.8: The Effect of Social Connectedness on Murder, 1980-2009, by Age-Race Group and Decade

	Dependent variable: Number of murders resulting in arrest for age-race group				
	All	Black Youth	Black Adults	Non-Black Youth	Non-Black Adults
	(1)	(2)	(3)	(4)	(5)
Coefficient on Log HHI, Southern Black Migrants by Decade					
1980-89	-0.210***	-0.761***	-0.355***	-0.200	-0.162
	(0.069)	(0.175)	(0.078)	(0.203)	(0.089)
1990-99	-0.224***	-0.305***	-0.247**	-0.458***	-0.278***
	(0.084)	(0.118)	(0.098)	(0.176)	(0.101)
2000-09	-0.148	-0.195	-0.086	-0.297	-0.227*
	(0.102)	(0.200)	(0.121)	(0.271)	(0.120)

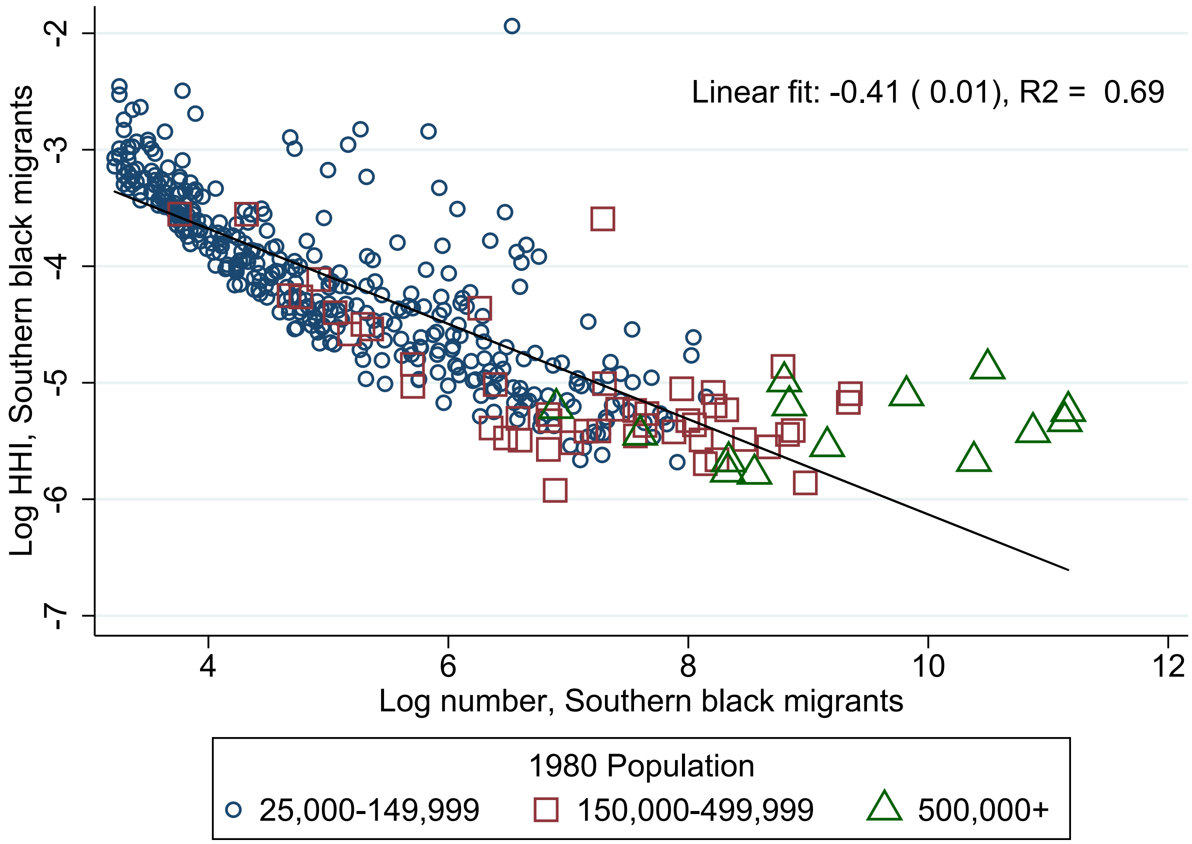
Notes: Table displays estimates of equation (3.12). Sample contains 298 cities that have less than 500,000 residents in 1980 and appear in at least five years of every decade from 1980-2009. Regressions include the same covariates used in Table 3.4. Standard errors, clustered at the city level, are in parentheses. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$
Sources: FBI UCR, Duke SSA/Medicare data, Census city data book

Table 3.9: The Role of Peer Effects in the Effect of Social Connectedness on Crime

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Peer effect parametrization							
$J_{11} = J_{22} = J_{33}$ (own-group)	0	0.25	0.25	0.25	0.5	0.5	0.5
$J_{12} = J_{21}$ (cross-group, black)	0	0	0.2	0.2	0	0.4	0.4
$J_{13} = J_{23}$ (cross-race, non-black on black)	0	0	0	0.67	0	0	0.67
$J_{31} = J_{32}$ (cross-race, black on non-black)	0	0	0	0.015	0	0	0.015
Implied peer effect elasticities							
$E_{11} = E_{22} = E_{33}$ (own-group)	0	0.25	0.25	0.25	0.5	0.5	0.5
$E_{12} = E_{21}$ (cross-group, black)	0	0	0.2	0.2	0	0.4	0.4
$E_{13} = E_{23}$ (cross-race, non-black on black)	0	0	0	0.1	0	0	0.1
$E_{31} = E_{32}$ (cross-race, black on non-black)	0	0	0	0.1	0	0	0.1
Implied peer effect multipliers							
m^s (blacks with ties to South)	1	1.33	1.44	1.48	2	5.56	8.92
m^n (blacks without ties to South)	0	0	0.38	0.43	0	4.44	7.81
m^w (non-black)	0	0	0	0.04	0	0	0.50
Percent change in murder rate due to one standard deviation increase in HHI, Southern Black Migrants							
City-level murder rate	-14.1	-14.1	-14.1	-14.1	-14.1	-14.1	-14.1
Murder rate among non-blacks	0	0	0	-5.2	0	0	-8.0
Murder rate among blacks	-28.3	-28.3	-28.3	-23.1	-28.3	-28.3	-20.3
Among blacks without ties to South	0	0	-9.9	-8.7	0	-24.2	-18.5
Among blacks with ties to South	-42.2	-42.2	-37.3	-30.1	-42.2	-30.3	-21.2
Direct effect of HHI	-42.2	-31.6	-26.0	-20.3	-21.1	-5.4	-2.4
Peer effect	0	-10.5	-11.3	-9.8	-21.1	-24.8	-18.8

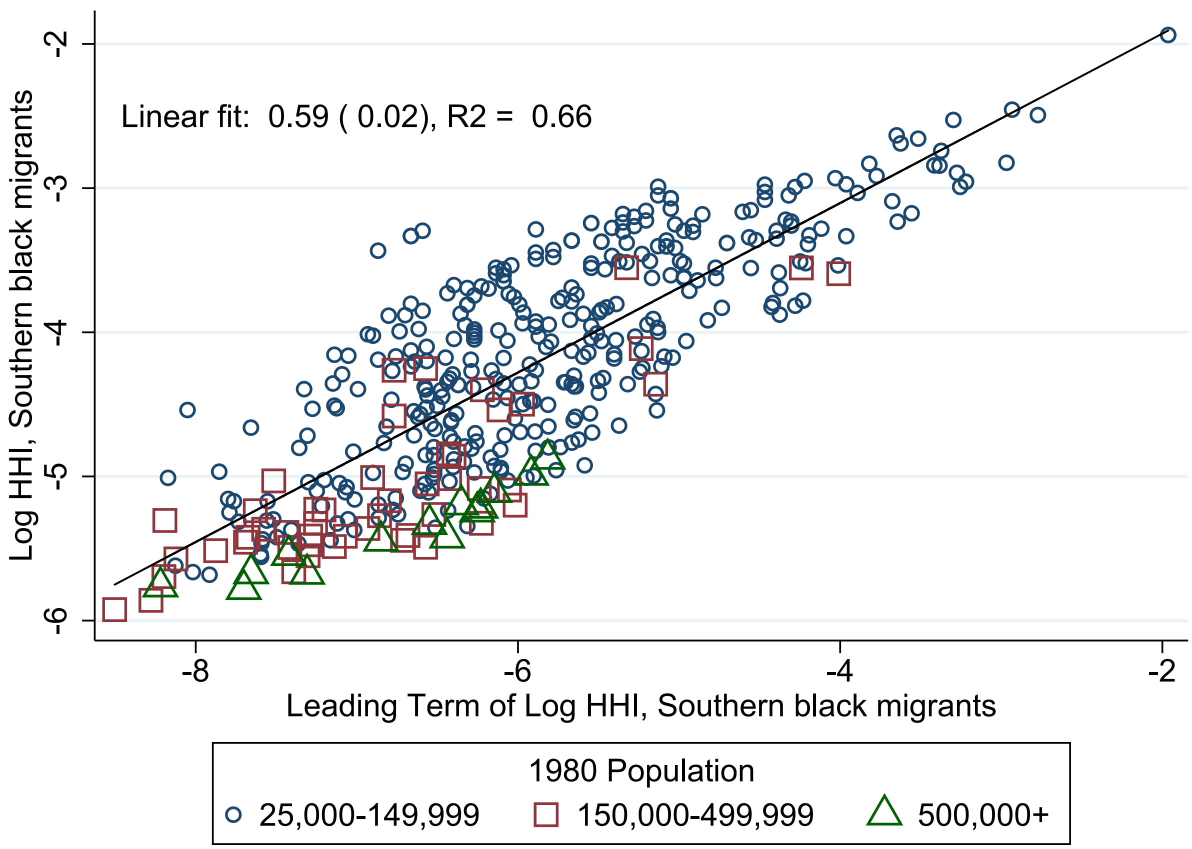
Notes: The top half of Table 3.9 describes the peer effect parametrizations that we consider. The bottom half decomposes the effect of a one standard deviation increase in social connectedness into changes in murder rates among different groups. See text for details.
Sources: FBI UCR, Duke SSA/Medicare data, Census city data book

Figure 3.1: The Relationship between Social Connectedness and the Number of Southern Black Migrants



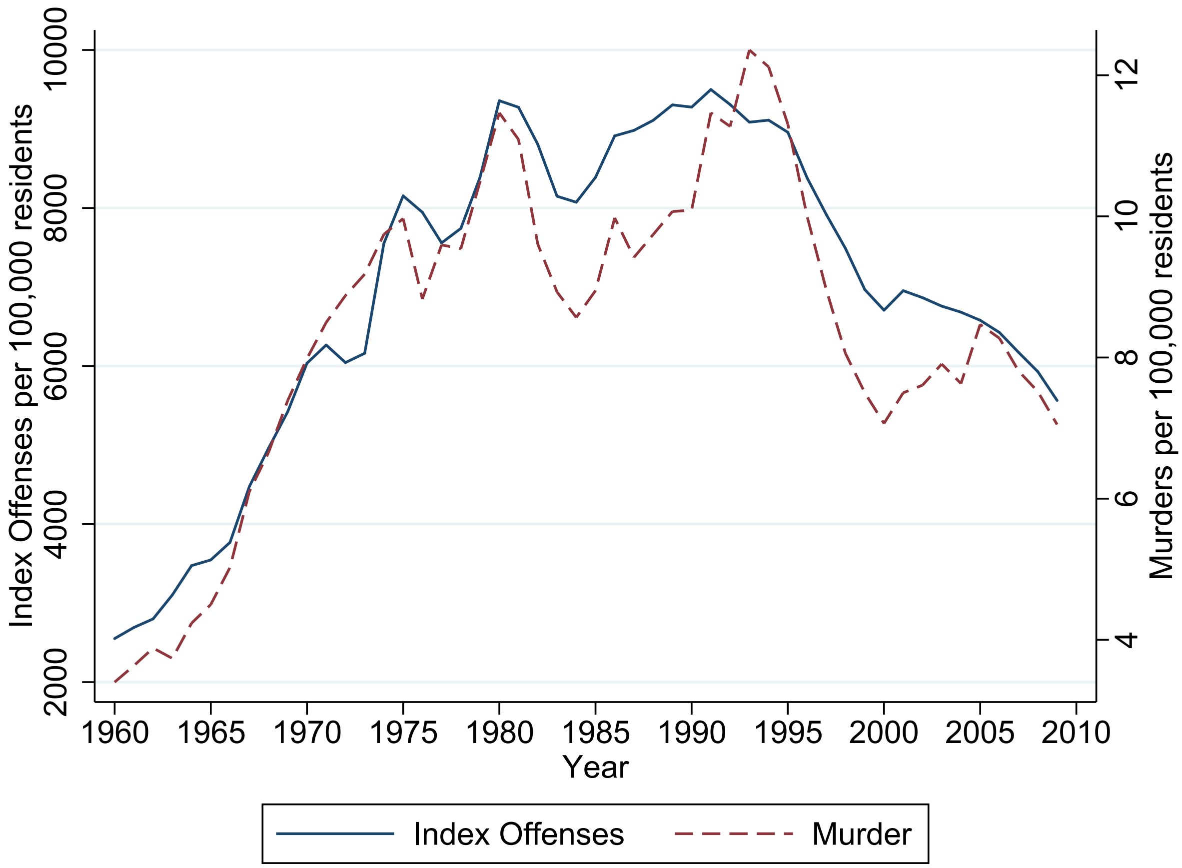
Notes: Figure contains 418 cities. Our main analysis sample excludes the 14 cities with at least 500,000 residents in 1980.
Source: Duke SSA/Medicare data

Figure 3.2: The Top Sending Town Accounts for Most of the Variation in Social Connectedness



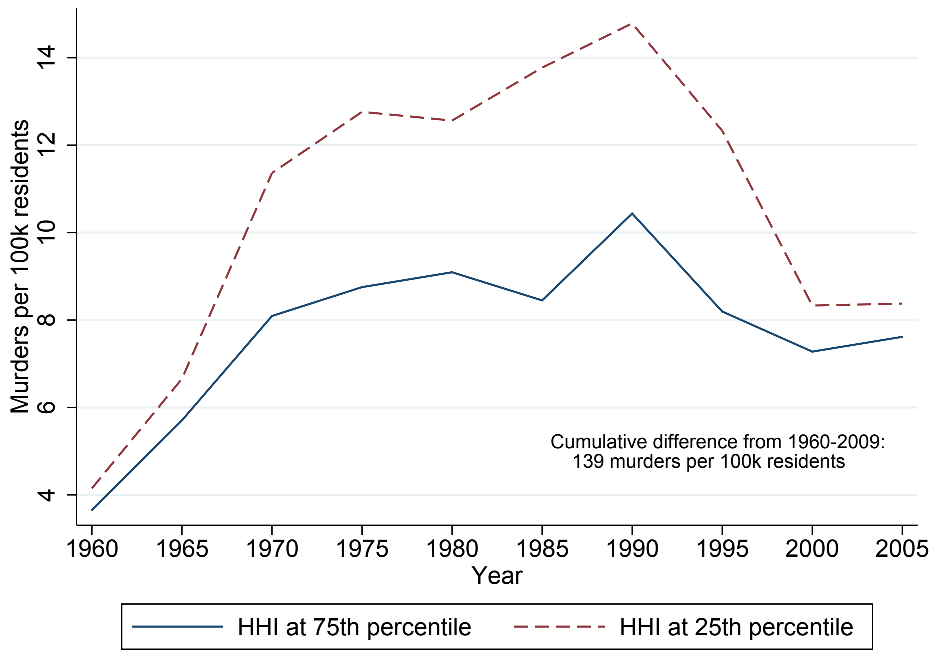
Notes: The leading term of HHI equals the log squared percent of migrants from the top sending town. Figure contains 418 cities. Our main analysis sample excludes the 14 cities with at least 500,000 residents in 1980.
Source: Duke SSA/Medicare data

Figure 3.3: The Evolution of Crime Rates Over Time

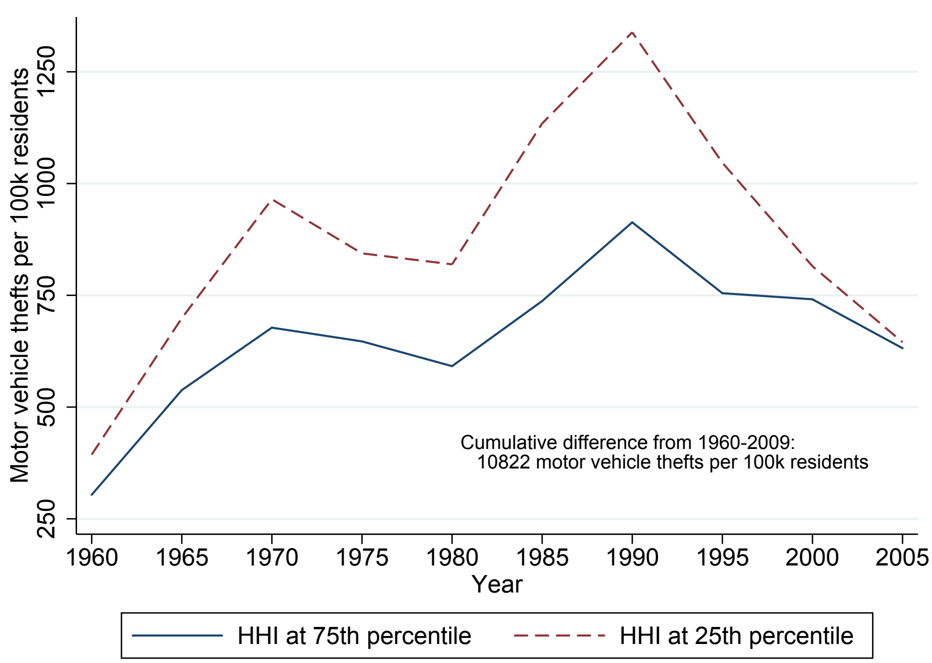


Notes: Index offenses include murder, rape, robbery, aggravated assault, burglary, larceny theft, and motor vehicle theft. Sample restricted to cities in our main analysis sample with less than 500,000 residents in 1980.
Source: FBI UCR

Figure 3.4: Social Connectedness and the Evolution of Crime Rates Over Time



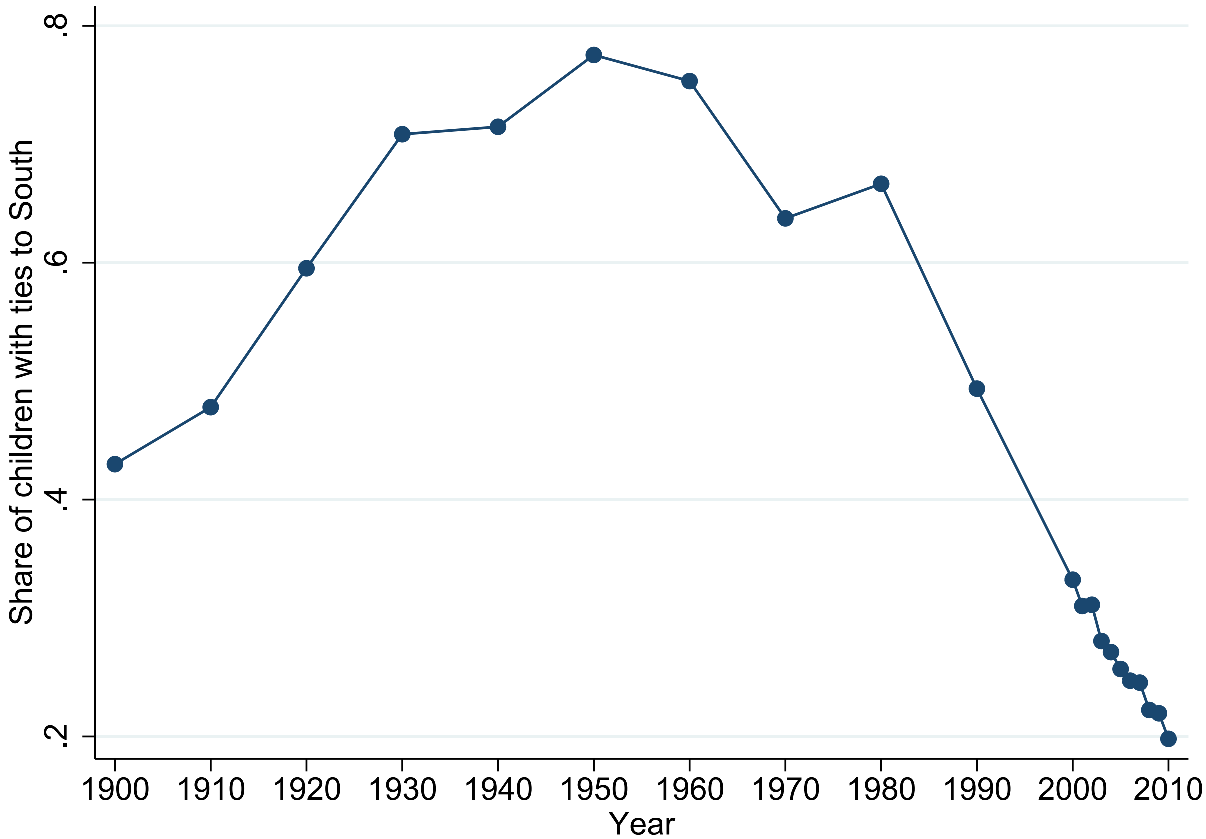
(a) Murder



(b) Motor Vehicle Theft

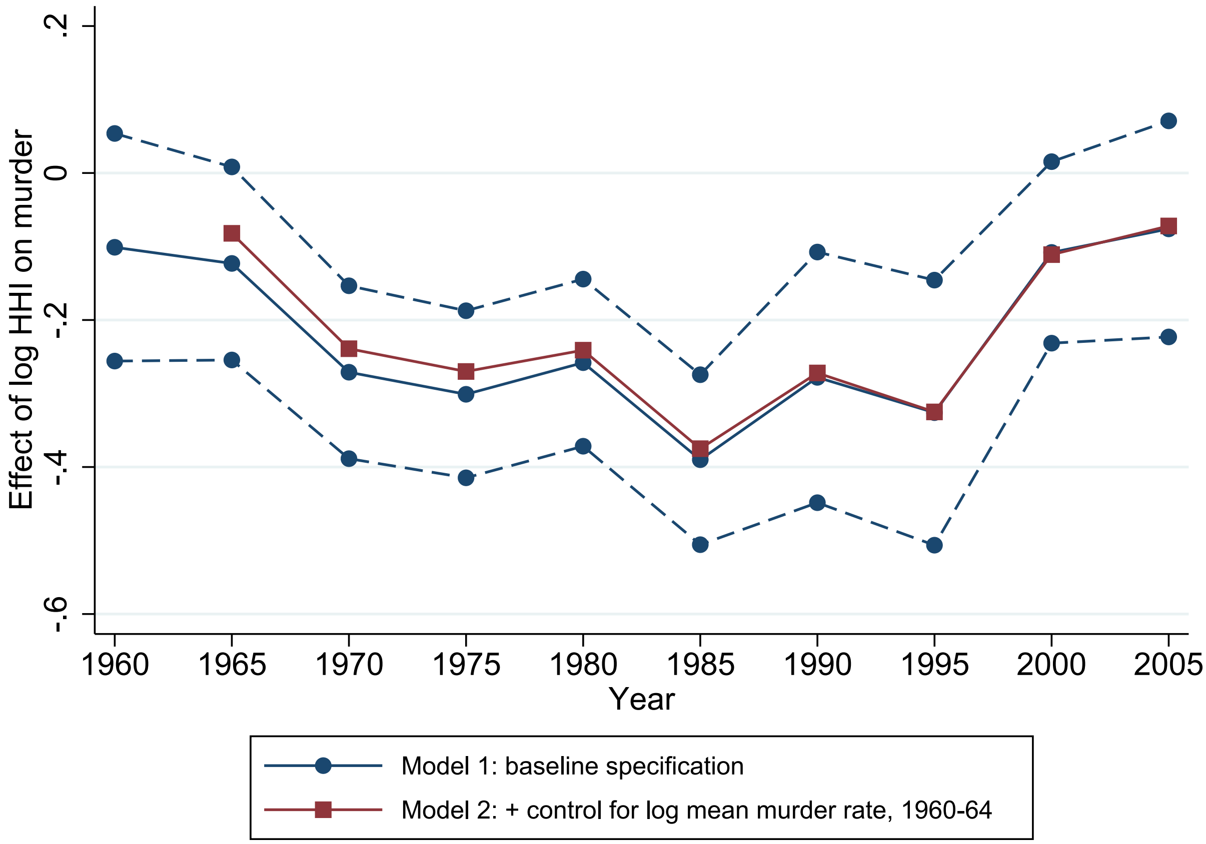
Notes: For each five year period from 1960-2009, we estimate equation (3.12) and take the level of covariates associated with the average crime rate. We then plot the murder rate associated with the 75th and 25th percentiles of HHI. Sources: FBI UCR, Duke SSA/Medicare data, Census city data book

Figure 3.5: The Share of African American Children Living in the North with Ties to the South



Notes: Figure plots the share of individuals age 14-17 who are living in the North, Midwest, or West regions who were born in the South or live in the same household as an adult born in the South.
Sources: IPUMS Decennial Census (1900-2000) and American Community Survey (2001-2010)

Figure 3.6: The Effect of Social Connectedness on Murder, Robustness to Controlling for 1960-1964 Murder Rate



Notes: Figure shows point estimates and 95-percent confidence intervals from estimating equation (3.12) separately for year 1960-64, 1965-69, and so on. Model 1 includes the same covariates used in Table 3.4, and model 2 additionally controls for the log mean murder rate from 1960-64.
Sources: FBI UCR, Duke SSA/Medicare data, Census city data book

APPENDICES

APPENDIX A

Appendix to Chapter 1

A.1 Imputing Employment in County Business Patterns Data

This section describes how I impute employment in Census CBP data.

CBP data always report establishment counts by county, industry, and establishment size, but frequently suppress employment at the county-by-industry level. From 1974-forward, the establishment size groups are 1-4, 5-9, 10-19, 20-49, 50-99, 100-249, 250-499, 500-999, 1000-1499, 1500-2499, 2500-4999, and 5000 or more employees.

I impute employment at the county-by-industry level using establishment counts and nationwide information on employment by establishment size. For establishments with fewer than 1000 employees, I impute employment as the number of establishments times average 1977 employment in the establishment size group, where the average comes from nationwide data across all industries.

Because nationwide CBP data do not report employment by establishment size group for establishments with at least 1000 employees, I assume that employment follows a log normal distribution, with mean μ and standard deviation σ, and estimate (μ, σ) using the generalized method of moments (GMM), as in Holmes and Stevens (2002). I estimate (μ, σ) using the following four moments:

$$p_1 = \Phi\left(\frac{\ln(1499)-\mu}{\sigma}\right) - \Phi\left(\frac{\ln(1000)-\mu}{\sigma}\right) \tag{A.1}$$

$$p_2 = \Phi\left(\frac{\ln(2499)-\mu}{\sigma}\right) - \Phi\left(\frac{\ln(1500)-\mu}{\sigma}\right) \tag{A.2}$$

$$p_3 = \Phi\left(\frac{\ln(4999)-\mu}{\sigma}\right) - \Phi\left(\frac{\ln(2500)-\mu}{\sigma}\right) \tag{A.3}$$

$$E[y] = \exp(\mu + \sigma^2/2) \tag{A.4}$$

where p_1 is the share of establishments (with at least 1000 employees) with 1000-1499 employees, p_2 is the share with 1500-2499 employees, p_3 is the share with 2500-4999 employees, $\Phi(\cdot)$ is the standard normal CDF, and $E[y]$ is average employment among establishments with at least 1000 employees. Equation (A.4) is possible because nationwide CBP data report total employment among establishments with at least 1000 employees.

I use equations (A.1)-(A.4) to estimate (μ, σ) with GMM, using the identity matrix as the weighting matrix.[1] Using 1977 data across all industries in the U.S., there are 1947 establishments with 1000-1499 employees, 1202 with 1500-2499 employees, 678 with 2500-4999 employees, and 275 with 5000 or more employees. Total employment among these establishments is 9,442,953. Consequently, $\hat{p}_1 = 1947/4102 \approx 0.475, \hat{p}_2 \approx 0.293, \hat{p}_3 \approx 0.165$ and $\hat{E}[y] \approx 2302$. The GMM estimates are $\hat{\mu} = 7.506$ and $\hat{\sigma} = 0.686$. Standard facts about the log-normal distribution imply that the imputed means for the four establishment size groups are 1247, 1952, 3414, and 7055.[2]

[1]When using equation (A.4) as a moment condition, data limitations prevent estimating standard errors or using the optimal weighting matrix. For example, one input in the variance and optimal weighting matrices is

$$\frac{1}{N}\sum_i \left[y_i^2 - 2y_i \exp(\mu + \sigma^2/2) + \exp(\mu + \sigma^2/2)\right],$$

where N is the total number of establishments and y_i is employment at establishment i. Because y_i is not observed, y_i^2 cannot be formed. An alternative would be to use only moment conditions (A.1) - (A.3).

[2]In particular, if $\ln(y) \sim \mathcal{N}(\mu, \sigma^2)$, then

$$E(y|a < y \le b) = E(y)\frac{\Phi(\sigma - a_0) - \Phi(\sigma - b_0)}{\Phi(b_0) - \Phi(a_0)}, \quad a_0 \equiv (\ln a - \mu)/\sigma, \quad b_0 \equiv (\ln b - \mu)/\sigma$$

$$E(y|y > a) = E(y)\frac{\Phi(\sigma - a_0)}{\Phi(-a_0)}$$

A.2 Relationship to Previous Work on the Persistence of the 1980-1982 Recession

Section 1.2 demonstrates that the 1980-1982 recession led to a persistent relative decline in earnings per capita, the employment-population ratio, and median family income at the county-level. This section details the relationship between my work and closely related papers by Feyrer, Sacerdote and Stern (2007) and Greenstone and Looney (2010) that use county-level data and study the same period. My finding that the 1980-1982 recession had persistent effects on counties agrees closely with Greenstone and Looney (2010), who document a persistent decline in income per capita and the employment-population ratio. However, my conclusion differs from that of Feyrer, Sacerdote and Stern (2007, hereafter FSS), who find rapid recovery of unemployment rates following auto and steel job losses. Two factors help explain this difference. First, the unemployment rate recovers more quickly than earnings per capita or the employment-population ratio; this is consistent with individuals adjusting their labor force participation more than their location. Second, FSS focus on auto and steel job losses, while I use all industries; the different sources of variation could lead to different effects, but the estimates are not precise enough to support sharp conclusions.

A.2.1 Relationship to Greenstone and Looney (2010)

Greenstone and Looney (2010) show that real income per capita and the employment-population ratio declined persistently for counties in the bottom 20 percent of the 1979-1982 income per capita change distribution, relative to the other 80 percent of counties. My Figure 1.1 very closely resembles their Figure 2, although I use earnings instead of income per capita, use the 50th instead of 20th percentile to define a severe recession county, use 1978 instead of 1979 as the pre-recession year, and normalize the two series to be equal in 1978.[3]

Relative to Greenstone and Looney (2010), I provide new evidence by examining the evolution of median family income from 1950-2000 and results at the commuting zone level. I characterize

[3]My Appendix Figure A.2 also resembles their Figure 3, subject to the same differences in construction.

the persistence of the recession more formally and show that the high degree of persistence holds within states. I also show the relationship between pre-existing industrial structure and the severity of the recession.

A.2.2 Relationship to Feyrer, Sacerdote and Stern (2007)

FSS study the effects of job losses in the auto and steel industries from 1977-1982 and find that county-level unemployment rates recovered within 5 years. FSS use OLS to estimate the regression

$$\Delta Y_c = \alpha + \beta \text{shock size}_c + \gamma_{d(c)} + \delta \text{MSA status}_c + \epsilon_c, \tag{A.5}$$

where ΔY_c is the change in some outcome over some horizon for county c. The shock size is the 1977-1982 employment change in the auto and steel industries divided by 1977 total employment. In some specifications, FSS use a binary measure which defines a shock county as one losing at least 2 percent of initial jobs. Equation (A.5) includes indicator variables for Census division, $\gamma_{d(c)}$, and a county's Metropolitan Statistical Area (MSA) status.[4] FSS limit their sample to counties with at least 10,000 residents in 1977.

In assessing the persistence of the auto and steel shock, FSS emphasize results where the dependent variable is the change in one minus the unemployment rate, or the employment-labor force ratio. I follow FSS in referring to this as the employment rate. This variable comes from Bureau of Labor Statistics (BLS) Local Area Unemployment Statistics data, which are constructed using the Current Population Survey, the Current Employment Statistics survey, and state unemployment insurance data. Besides the unemployment rate, BLS data also report estimates of the number of people who are employed, unemployed, and in the labor force. Data are available annually from 1976-forward and are adjusted to reflect county of residence. The BLS states that, "[a]lthough substate data for 1976-89 exist in archived files, they are not consistent with data for the 1990s, nor are they consistent within the pre-1990 period. Moreover, substate estimates for years prior to 1990 are no longer official BLS data" (Bureau of Labor Statistics, 1998).[5] From 1976-1984,

[4]I use the 1999 MSA definitions, which appear to be consistent with the MSAs that FSS list in their Table 1.

[5]Official data, for 1990-forward, are available on the BLS website. I received the 1976-1989 data from the BLS

BLS constructed county-level variables by disaggregating labor market area statistics, assuming a uniform employment-population ratio throughout a labor market area (Bureau of Labor Statistics, 1998).[6]

In principle, several reasons could explain why FSS arrive at a different conclusion than I do. First, they emphasize results based on the unemployment rate, while I emphasize results for earnings per capita and the employment-population ratio.[7] The unemployment rate might recover more quickly than other outcomes if individuals respond to the shock by exiting the labor force.[8] Second, FSS focus on job losses in the steel and auto industries, while I focus on job losses in all industries. Third, the comparison group in FSS includes counties with a high share of employment in mining, which experienced a countercyclical boom-bust cycle during the 1970's and 1980's. My 2SLS estimates reveal less persistence when including these counties (see Appendix Table A.4). Finally, FSS exclude counties with less than 10,000 residents in 1977 and include division and MSA fixed effects, while I include all counties and include state fixed effects.

I am able to closely replicate the shock size variable used by FSS. While FSS do not fully describe some data processing details, I believe that I have inferred these details by successfully replicating their Table 1, which helpfully lists the shock counties and associated job losses.[9] I believe FSS use County Business Patterns (CBP) employment counts to measure the employment change in the auto and steel industries (i.e., the numerator of the shock size). This approach could be problematic, as CBP data frequently suppress employment counts to protect respondent confidentiality, and FSS appear to treat suppressed employment as zero employment.[10] A potentially

via e-mail.

[6]Previous studies question how much valuable information county-level unemployment rate data contain, especially conditional on county and year fixed effects (Bartik, 1996; Hoynes, 2000).

[7]FSS find a persistent relative decrease in income per capita in shock counties (see their Table 11), as do Greenstone and Looney (2010) and I. Consequently, their findings might not be best summarized by the claim in the introduction that "Rust Belt counties and MSAs recovered quickly on certain dimensions like unemployment and income per capita" (Feyrer, Sacerdote and Stern, 2007, p. 42).

[8]Using county-level BLS data from 2000-2010, Foote, Grosz and Stevens (2015) find that mass layoffs lead to greater reductions in labor force than population. In principle, non-classical measurement error in the BLS unemployment rate data could also contribute to differences in the results.

[9]The only difference between Table 1 of FSS and my replication is that I have Neosho, KS, Laclede, MO, and St. Louis, MO as shock counties, but FSS do not.

[10]In 1977, 1,144 counties had at least one establishment in the steel industry (SIC 3300), and 861 of these counties (75 percent) had suppressed employment. In the auto industry (SIC 3700), 1,515 counties had at least one establish-

more accurate approach is to use establishment counts, which are never suppressed, and impute employment as described in Appendix A.1 and Holmes and Stevens (2002). I believe that FSS measure 1977 total employment from BLS data.

I have not been able to replicate the non-shock counties used by FSS. In Table 2, FSS list 66 shock counties (62 of which have non-missing 1977 population) and 1,373 non-shock counties (1,253 of which have non-missing 1977 population). My sample contains 69 shock counties and 2,257 non-shock counties (all of which have non-missing 1977 population).

Appendix Figure A.4 displays the differences that arise when using CBP employment versus establishment counts to measure the shock size. Panel A shows the bivariate relationship for the 2,326 counties with at least 10,000 residents in 1977 (the same sample restriction used by FSS). Employment suppression is visible in the cases where the shock size based on employment counts equals 0, while the shock size based on establishment counts (horizontal axis) does not. The linear correlation between the two measures is 0.2. Panel B displays an analogous figure for all counties. The basic pattern is similar, but the linear correlation falls to 0.01. Classical measurement error does a poor job of describing the relationship between these two variables, as the employment count shock size varies less than the potentially better-measured establishment count shock size.[11]

Appendix Table A.1 shows that I can closely approximate the results of FSS on how the auto and steel shock affected the employment rate. The table reports estimates of equation (A.5) where the dependent variable is the change over different horizons in the employment rate (i.e., one minus the unemployment rate). Panels A and C repeat Tables 3 and 4 of FSS, and Panels B and D report my estimates. The point estimates and standard errors are extremely similar, although the number of observations and R^2 differ.[12]

Appendix Table A.2 examines different dependent variables using the FSS specification and assesses the impact of using CBP establishment counts to construct the shock size. Panel A,

ment, and 1,167 counties (77 percent) had suppressed employment.

[11] When limiting to counties with at least 10,000 residents in 1977, the variance of the establishment count shock size is over five times that of the employment count shock size. When not making this population restriction, the multiple is over two.

[12] Standard errors in Appendix Table A.1 are robust to heteroskedasticity, but are not clustered. I do not know how FSS estimate their standard errors.

which uses CBP employment counts to construct the shock size, demonstrates that a negative shock reduced the employment rate, employment-population ratio, and earnings per capita from 1977-1982. For example, the point estimate in column 1 indicates that a decrease in auto and steel employment equal to 1 percent of a county's initial employment decreased the employment rate by 0.2 percent from 1977-1982.[13] The employment rate elasticity is less than half that of other outcomes. Panel B presents results using CBP establishment counts to measure the shock size. The results in Panels A and B differ somewhat, especially for dependent variables measured using BEA data. Panels C and D examine the change in outcome variables from 1977-1987. Panel C, which uses CBP employment counts as in FSS, cannot reject complete convergence of the employment rate, but finds persistent effects on the employment-population ratio and earnings per capita. Most of the point estimates are attenuated and indistinguishable from zero in Panel D, which uses CBP establishment counts, but the upper range of the confidence intervals admit moderate effects.[14]

Appendix Table A.3 shows that (1) the employment rate appears to recover more quickly than the employment-population ratio or earnings per capita and (2) the effects of the FSS shock are typically attenuated and estimated with less precision than the effects of the 1980-1982 recession shock that I use. Panel A displays results from a specification similar to equation (A.5), but I do not control for MSA status.[15] Panel B measures the shock size using CBP establishment counts, and Panel C includes counties with fewer than 10,000 residents in 1977. Estimates are attenuated when using CBP establishment counts, but are very similar when including all counties. Panel D replaces the FSS shock size with the 1978-1982 change in log real earnings per capita. The coefficient on the employment rate is a precisely estimated 0, but there are lasting effects on the employment-population ratio and earnings per capita. Panel E uses the predicted log employment change from 1978-1982 as an instrumental variable. Panels F-H repeat Panels C-E, but exclude the

[13]This estimate is similar to the analogous estimate in FSS (see column 1 of their Table 6, Panel A).

[14]There are some differences between the point estimates in columns 2 and 3 of Appendix Table A.2. The dependent variable in both columns is the ratio of employment to population age 15 and older, with employment in column 2 coming from BLS data and in column 3 from BEA data. BLS data refer to place of residence and count the number of people employed, while BEA employment data refer to place of work and count the number of jobs. Both series are derived from the same underlying data, but the BEA adjusts for sectors not covered by unemployment insurance, uses additional data to measure employment in certain industries, and adjusts for misreporting.

[15]I cluster standard errors by state in Appendix Table A.3 as in my preferred specification.

526 counties with at least 5 percent of 1976 employment in the mining sector, which experienced a countercyclical boom-bust cycle. Estimates using the FSS shock variable in Panel F are somewhat imprecise and indistinguishable from zero, while the OLS and 2SLS estimates in Panels G and H show significant effects of the change in log earnings per capita on all variables, with much smaller effects on the employment rate. To compare the FSS shock size and the predicted log employment change in all industries, Panel I reports results of instrumenting for the 1978-1982 change in log earnings per capita with the shock size based on CBP establishment counts.[16] The rescaled estimates are typically within one standard error of the point estimates in Panel H, but the 2SLS estimates using the shock size are very imprecise.

Appendix Figure A.5 provides additional evidence on differences between the predicted log employment change in all industries and the shock size variable used by FSS. When using CBP employment counts (Panel A) or establishment counts (Panel B), there are many counties which experience no job loss in the steel or auto industries, but are predicted to experience considerable job loss in other industries. These variables do not appear to capture the same underlying phenomenon. While the auto and steel industries are important and interesting, the recession affected many other industries as well (see Table 1.1).

A.3 Additional Results on the 1980-1982 Recession

A.3.1 The Persistence of the Recession

Figure 1.1 shows that the 1980-1982 recession led to a persistent decrease in earnings per capita for negatively affected counties. This section provides a more formal characterization of the persistence of the recession.

A simple way of measuring the persistence of the recession is by relating the 1978-1992 and 1978-1982 changes in log real earnings per capita,

$$\ln(E_{c,1992}) - \ln(E_{c,1978}) = \alpha + \beta\left(\ln(E_{c,1982}) - \ln(E_{c,1978})\right) + v_c, \tag{A.6}$$

[16]The first stage slope coefficient is 0.270 (0.107), with an F-statistic of 6.41, so there is some concern about a weak instrument.

where $E_{c,t}$ is real earnings per capita for county c in year t. In equation (A.6), the average degree of persistence is captured by β, with full persistence represented by $\beta = 1$ and no persistence represented by $\beta = 0$. However, equation (A.6) has the unattractive property that, even if earnings per capita displays no serial correlation, the model implies a non-zero degree of persistence, $\beta = 0.5$. This arises because $\ln(E_{c,1978})$ appears on both the left and right hand sides of equation (A.6) and occurs even in the absence of measurement error.

To quantify the average degree of persistence, I estimate the regression

$$\ln(E_{c,1992}) = \alpha + \beta \ln(E_{c,1982}) + \gamma \ln(E_{c,1978}) + X_c\delta + v_c. \tag{A.7}$$

X_c includes state fixed effects and the 1950-1970 change in log real median family income in county c, which I include in my preferred specification for estimating long-run effects on children.[17]

Table A.4 shows that the 1980-1982 recession led to a statistically and economically significant persistent decrease in earnings per capita. The OLS estimate of $\hat{\beta}$ in column 1 indicates that, conditional on earnings per capita in 1978 and X_c, a ten percent decrease in earnings per capita from 1978-1982 leads to 6.4 percent lower earnings per capita in 1992.[18] Column 2 reports 2SLS estimates using the predicted log employment change from 1978-1982 in all industries as an instrument. I exclude the 526 counties with at least 5 percent of 1976 employment in the mining sector to limit the countercyclical boom-bust cycle in this sector. A 10 percent decrease in earnings per capita from 1978-1982 leads to 13.2 percent lower earnings per capita in 1992. Column 3, which uses the same instrument but includes counties with a large mining employment share, shows less persistence, as expected. Column 4, which uses the predicted log employment change in manufacturing alone, also reveals full persistence. Results are similar when examining the log

[17]Equations (A.6) and (A.7) are equivalent when $\beta + \gamma = 1$ and X_c is included in equation (A.6). However, equation (A.7) eliminates the bias that arises from estimating equation (A.6).

[18]This interpretation is clear when rewriting equation (A.7) as

$$\ln(E_{c,1992}) = \alpha + \beta(\ln(E_{c,1982}) - \ln(E_{c,1978})) + (\gamma + \beta)\ln(E_{c,1978}) + X_c\delta + v_c.$$

employment-population ratio (Appendix Table A.5). The degree of persistence is similar for years 1987, 1992, and 1997, but economic activity declined further in 2002, 2007, and 2012 in counties which experienced a more severe 1980-1982 recession (Appendix Table A.6). Possible explanations for the decay in the 2000's include the long-run decline in human capital associated with the 1980-1982 recession or the long-run adjustment of employers (Dix-Carneiro and Kovak, 2016).[19]

A.3.2 The Effect of the 1980-1982 Recession on Housing Prices

This section shows that the median price of housing fell from 1980-1990 in counties with a more severe recession, but by less than the decrease in median income.

The price of housing and other local goods could decrease after the recession, mitigating the earnings decrease. To see this, suppose that household utility, $u(x, y)$, depends on consumption of a numeraire traded good x and a non-traded good y with local price p. The household budget constraint is

$$(1 - \tau)w = x + py, \tag{A.8}$$

where τ is the marginal tax rate and w is family earnings. For simplicity, I assume that labor supply is fixed. The expenditure function is $e(p, u) = (1 - \tau)w$, where u is the level of utility. Using Shepherd's Lemma and rearranging, it is straightforward to show that a household will be indifferent to a change in earnings and local prices as long as

$$(1 - \tau)\hat{w} = s_y\hat{p}, \tag{A.9}$$

[19] In principle, the decline in the 2000's could also be due to additional negative shocks, but Figures 1.1 and A.1 provide little support for this interpretation.

The shock to local labor markets from increased Chinese import competition studied by Autor, Dorn and Hanson (2013) is only weakly correlated with the severity of the 1980-1982 recession. A one standard deviation increase in the average of 1990-2000 and 2000-2007 increase in import competition is associated with a 0.5 percent decrease in earnings per capita from 1978-1982 and a 0.8 percent decrease in predicted employment. A one standard deviation increase in average predicted import competition is associated with a 0.8 percent decrease in earnings per capita and a 1.0 percent decrease in predicted employment. The average change in log earnings per capita is -0.071, and the standard deviation is 0.114. The average predicted log employment change is 0.037, and the standard deviation is 0.083. These calculations come from matching my county-level data to the CZ-level data from Autor, Dorn and Hanson (2013), estimating regressions with state fixed effects, and calculating unweighted summary statistics.

where the proportional change in earnings is $\hat{w} \equiv dw/w = d\ln(w)$, the proportional change in the price of the non-traded good is $\hat{p}$, and $s_y \equiv py/w$ is the share of earnings spent on the non-traded good. After accounting for taxes and deductions, a reasonable approximation is $\tau = 0.32$ and $s_y = 0.33$ (Albouy, 2012). Consequently, the cost of the non-traded good would need to fall in proportional terms by around twice as much as the fall in earnings for households to be indifferent.[20]

Appendix Table A.7 shows that the median price of housing fell from 1980-1990 in counties with a more severe recession, but by less than the decrease in median income. The table reports 2SLS regressions of the 1980-1990 change in log median family income, log median rent, and log median house value on the 1978-1982 change in log earnings per capita. As elsewhere, the regressions control for state fixed effects and the 1950-1970 change in log median family income. Panel A excludes counties with a high mining employment share and uses the predicted log employment change in all industries as the instrumental variable. A 10 percent decrease in earnings per capita from 1978-1982 leads to a 10.0 percent decrease in median family income from 1980-1990, but only a 7.2 and 7.8 percent decrease in median rent and median house value. As expected, these patterns are attenuated when including counties with a high mining employment share in Panel B. This evidence, especially in Panel A, is broadly consistent with the results of Bound and Holzer (2000), who find that wages fell by more than house prices using cross-metro regressions from 1980-1990.[21]

[20]This simple analysis could be extended so that households also value local quality of life amenities (Albouy and Stuart, 2016). If a decrease in labor demand does not affect quality of life, then equation (A.9) remains the relevant condition. If a decrease in labor demand also decreases quality of life, then households would require an even greater decrease in house prices to remain indifferent.

The analysis also could be extended to the model described in Section 1.3, where parents purchase traded and non-traded goods for their children and allocate their time between market work, investment in child human capital, and leisure. In this case, the relevant indifference condition is

$$t_{\text{work}}(1-\tau)\hat{w} = s_y\hat{p},$$

where $t_{\text{work}} \in [0, 1]$ is the share of time allocated to market work and $\hat{w}$ is the proportional change in the wage. For a given decrease in wages, parents require a smaller non-traded price decrease to remain indifferent because the price of time with children and leisure falls.

[21]Bound and Holzer (2000) use price indices for 26 large metropolitan statistical areas and find that a 10 percent decrease in labor demand is associated with a 2.6 percent decrease in local price levels (see their footnote 28). The same decrease in labor demand leads to a 4.2 percent decrease in wages of college graduates and a 6.9 percent decrease

A.3.3 The Effects of the 1980-1982 Recession on Commuting Zones

It is of some interest to examine patterns for commuting zones (CZs), which have been used in previous work to approximate local labor markets. Appendix Figures A.8 and A.9 display the evolution of mean real earnings per capita and employment-population ratios for CZs with an above and below median decrease in log real earnings per capita from 1978-1982.[22] Appendix Figure A.8 shows that mean real earnings per capita in CZs with a more and less severe recession evolved similarly before 1979, but diverged persistently after 1982; this pattern is very similar to the county-level results in Figure 1.1. Appendix Figure A.9 shows that employment-population ratios converged within a decade, in contrast to the lack of convergence seen at the county-level (Appendix Figure A.2).[23] Appendix Figures A.8 and A.9 suggest that there was greater scope for the recovery of jobs across CZs than counties, but that the incremental jobs offered lower earnings. Understanding the household- and firm-level behavior that generate these patterns, and the distinction between counties and CZs, is an interesting direction for future work.

A.4 Effects on Local Government Expenditures and Revenues

This section examines the effects of the 1980-1982 recession on local government expenditures and revenues, which could affect human capital development in childhood. I find that expenditures per capita fell starting in 1992 in counties that experienced a more severe recession, but there is little evidence of a decrease before then, likely due to higher federal transfers. The decline in expenditures is driven by spending on welfare and health, and not education.

To examine the effect of the recession on local government expenditures and revenues, I estimate event study regressions similar to equation (1.2), where the dependent variable is log real

in wages of non-college graduates (Table 3). Other authors find different results. Using a different source of variation, Bartik (1991) finds that decreases in labor demand have similar effects on local prices and wages. Blanchard and Katz (1992) find that median house prices initially decline more than wages, but that both approximately converge within 12 years (Figures 12 and 15). Notowidigdo (2013) finds that a decrease in labor demand reduces income per adult slightly more than the price of housing, but reduces wages by less than the price of housing (Tables 2 and 4).

[22] I aggregate county-level data to 1990 CZ definitions using the crosswalk provided by Autor and Dorn (2013).

[23] Using state-level data, Yagan (2016) finds employment-population ratio convergence from the 1980-1982 recession in 8 years (see his Figure A.1.D).

expenditures or revenues.[24] I use data from the Census of Governments, which contains information on expenditures and revenues for all government units in years that end in a "2" or "7."[25] I collapse all government units to the county level for years 1972, 1977, 1982, 1987, 1992, and 1997. I normalize the interaction between year 1977 and the severity of the recession to equal zero. I estimate the model by 2SLS, using the predicted log employment change from 1978-1982 in all industries as the IV. To remove the countercyclical boom-bust cycle experienced by the mining sector, I limit the sample to the 2,550 counties with no more than 5 percent of 1976 employment in the mining sector. I control for log population and the share of the population age 0-4, 5-19, and 20-64, which could affect the amount and composition of expenditures and revenues.

Appendix Table A.8 shows that the recession had little effect on expenditures in the short-run, but is associated with reductions from 1992-forward. I focus on general direct expenditures, which represent all expenditures besides those for liquor stores, utilities, insurance trusts, or intergovernmental transfers, and amount to 89 percent of total expenditures in 1977.[26] The results in column 1 provide little evidence that the recession reduced expenditures per capita in 1982 or 1987, but there is a significant decrease in expenditures in 1992 and 1997. A 10 percent decrease in earnings per capita from 1978-1982 is associated with an 11.2 percent reduction in expenditures in 1992 and an 8.8 percent reduction in 1997. Columns 2-6 demonstrate that the long-run reduction is not driven by education or public safety spending, which account for 59 percent of spending in 1977, but instead by welfare and health, infrastructure, and other purposes.[27] Columns 7-8 show that both

[24] In a very small number of instances, a county reports 0 expenditures or revenues for the outcomes I examine. To maintain a constant sample, I use the inverse hyperbolic sine, $\ln(y+\sqrt{1+y^2})$, instead of $\ln(y)$ throughout (Burbridge, Magee and Robb, 1988). The log and inverse hyperbolic sine yield very similar coefficients in linear regression models when y is sufficiently large.

[25] I downloaded these data from the NBER website, with thanks to Michael Greenstone for making them available. I exclude the five New York City counties from the analysis because they are combined into a single geographic unit.

[26] I exclude liquor stores, utilities (water supply, electric power, gas supply, and mass transit), and insurance trusts to focus on government activities most likely to affect children, but results are similar when including these categories. I exclude intergovernmental expenditures to avoid double counting, which could arise when a county government gives money to a school district, which then spends the money on teachers' salaries. The grouping of expenditures and revenues in Appendix Tables A.8 and A.9 is similar to that used by Bartik et al. (2016).

[27] Education expenditure purposes include elementary and secondary education, higher education, and libraries. Public safety expenditure purposes include police, correctional facilities, fire, judicial and legal, and protective inspection and regulation. Welfare and health expenditure purposes include welfare, health and hospital, transit subsidies, and housing and community development. Infrastructure expenditure purposes include airport, total highway, parking, sewerage, solid wage management, and water transport and terminals. Examples of other expenditure purposes are

current and capital expenditures decreased in the 1990's; the point estimates indicate an earlier and larger decrease in capital spending.

Appendix Table A.9 provides suggestive evidence that intergovernmental transfers initially offset the decrease in tax revenues after the recession. As seen in column 1, there is a significant decrease in general direct revenues from 1992-forward.[28] Underlying this is an immediate decrease in tax revenue (column 2), possibly offset by an increase in intergovernmental transfers in 1982 and 1987 (column 4). Column 5 shows that property taxes, which account for 33 percent of general direct revenue and 89 percent of tax revenue, drive the decrease in total tax revenues. Columns 6-8 suggest that offsetting intergovernmental transfers came from federal and local, as opposed to state, governments.

Unfortunately, the results in Appendix Tables A.8 and A.9 are estimated with sufficient imprecision that uncertainty remains about the evolution of government finances over time and the relative importance of different types of expenditures and revenues. These results do not exploit heterogeneity across states in the severity of the recession, initial asset holdings, or restrictions on local government finances. It would be interesting to explore these dimensions further.

A.5 Matching NUMIDENT Data to Counties

This section describes the procedure used to match the Social Security Administration NUMIDENT file to FIPS county codes. The procedure described here was developed alongside Martha Bailey, Evan Taylor, and Reed Walker. Researchers with access to confidential Census data can read a technical memo with more information on this procedure and will be able to access the code and output from this procedure (Taylor, Stuart and Bailey, 2016).

We seek to match information on individuals' place of birth to county FIPS codes. The NUMIDENT file, which draws on Social Security card applications, contains a 12-character string identifying the place of birth (city and/or county) and a 2-character string identifying the state of

financial administration, central staffing, and parks and recreation.

[28]As expected given balanced budget requirements, the change in expenditures in Appendix Table A.8 approximately mirror the change in revenues in Appendix Table A.9.

birth postal code.[29] We identify a set of target locations using U.S. Geological Survey data on current and historical locations from the Geographic Names Information System (GNIS).[30] GNIS data contain place names and county FIPS codes.

Several challenges prevent exact, unique matching of the NUMIDENT 12-character strings to GNIS counties. First, some place names in a state are indistinguishable with only 12 characters.[31] Second, place names are frequently misspelled. Third, the place of birth string sometimes contains acronyms and abbreviations, such as "Mnpls" for Minneapolis. Fourth, some NUMIDENT records contain the wrong postal code for their state of birth (e.g., "Anchorage, AL" where "AL" is the wrong abbreviation for Alaska).

Our algorithm yields four broad categories of matches. Each step proceeds sequentially and only applies to NUMIDENT strings not previously matched. In a preliminary processing step, we correct for common acronyms and abbreviations by hand for any string that occurs more than 50 times in the NUMIDENT data for birth cohorts 1950-1985. First, we obtain exact matches for correctly spelled place names that can be uniquely identified in a birth state with 12 characters. Second, we obtain "duplicate" matches for correctly spelled place names that can, in principle, be identified uniquely in 12 characters. We assign individuals to a single birth county if at least 75 percent of the exact matches are to a single county, and we assign multiple birth counties otherwise.[32] Third, we use hand matches from Isen, Rossin-Slater and Walker (Forthcoming), described in their Appendix C. Fourth, we use probabilistic matching algorithms.[33] Finally, we

[29] We use the 2012 version of the NUMIDENT file, accessed through the Michigan Census Research Data Center. For individuals born outside the United States, the 2-character string identifies the country of birth.

[30] We restrict attention to geographic features that are plausibly populated (those with a Populated Place, Census, or Civil feature class) or have a federal location code.

[31] For example, there are three different Populated Places in North Carolina beginning with "Bells Crossroads" located in different counties. Repeated place names pose less of a problem if the place name has less than 12 characters. For example, there are two places named Arcadia in North Carolina: one in Davidson County and the other in Forsyth County. These can be distinguished if "Arcadia Davi" or "Arcadia Fors" appear in the NUMIDENT.

[32] For example, a person born in North Carolina who writes "Arcadia Fors" or "Arcadia Davi" is matched to the correct Arcadia (in Forsyth or Davidson county) in the exact matching step. However, if an individual writes "Arcadia," we do not know in which Arcadia they were born. If at least 75 percent of the exact Arcadia matches are attributed to one county, then we match "Arcadia" to that county.

[33] In the probabilistic matching step, we only match NUMIDENT strings to GNIS places that have census codes to control the number of false positive matches. We first use the Stata command reclink2 (Wasi and Flaaen, 2015), with the tolerance set to 0.1, to obtain a set of potential matches for each NUMIDENT string. We then use the Stata command jarowinkler (Feigenbaum, 2015) to select the best match as the one with the highest Jaro-Winkler score

hand check all strings that are matched in the probabilistic step, disagree with the match found in Isen, Rossin-Slater and Walker (Forthcoming) algorithm (but were not hand checked by them), and have at least 50 occurrences in the NUMIDENT file.

Appendix Table A.10 summarizes match rates for individuals observed in the 2000 Census and 2001-2013 ACS. I limit the sample to individuals who were born from 1950-1980 and were age 25-64 at the time of the survey. I also limit the sample to individuals with non-imputed values of sex, age, race, and state of birth, and who report being born in the U.S on the census survey.[34] 95.9 percent of the sample has a non-missing protected identification key (PIK), which is the anonymous identifier used to link Census and SSA data. Of these individuals, 99.6 percent have a PIK which is not duplicated within a survey year. We identify a unique birth county for 93.6 percent of the individuals with non-duplicated PIKs. Ultimately, these restrictions leave 89.4 percent of the initial sample. The majority of matches, 80.4 percent, are exact matches, while 11.0 percent are duplicates, 5.1 percent are matched probabilistically, and 3.5 percent are hand matches.

A.6 Pre-Recession Migration is Not Correlated with the Severity of the Recession

This appendix shows that there is little evidence that pre-recession out-migration propensities are correlated with the severity of the recession. This finding is not necessary for the measurement error approach described in Section 1.4.3, but provides additional information about pre-recession migration patterns.

Based on publicly available 1980 Census data (Ruggles et al., 2015), 2SLS regressions do not reveal a significant relationship between children's 1975-1980 migration and the recession severity in their 1975 commuting zone (CZ): a 10 percent decrease in earnings per capita from 1978-1982 is associated with a 3.8 (standard error: 3.4) percentage point increase in the probability of moving across CZs.[35] There is also no evidence of a significant relationship between the probability that

among the potential matches. If no potential match has a Jaro-Winkler score of at least 0.8, then the string remains unmatched. If multiple places have the same Jaro-Winkler score, then this step matches to each place.

[34] I use similar restrictions in my analysis.

[35] The regression includes birth state-by-age fixed effects, plus indicator variables for race and sex. I estimate the

a child lives outside his or her birth state and the severity of the recession in their 1975 CZ: a 10 percent decrease in earnings per capita is associated with a 7.1 (11.9) percentage point increase in the probability of living outside one's birth state.[36]

A.7 Additional Support for the Empirical Strategy from Birth Certificate Data

To further examine the validity of my empirical strategy, I examine whether the pre-recession evolution of infant mortality, parental characteristics, and infant health are correlated with the severity of the 1980-1982 recession. I do not detect a meaningful relationship, which provides evidence that my estimates of the long-run effects of the recession on children are not driven by differential pre-recession trends in infant health or parental characteristics.

I examine the evolution of the infant mortality rate (deaths per 1,000 births) by estimating regressions similar to equation (1.2). The regression includes fixed effects for county of residence and state-by-birth year, plus controls for birth year interacted with the 1950-1970 change in log median family income.[37] My sample contains individuals born from 1950-1979. I normalize the interaction between the severity of the recession and birth year to equal 0 for individuals born in 1950, and I aggregate the remaining interactions into three-year bins. I use the predicted log employment change as the instrumental variable, and exclude the 526 counties with at least 5 percent of 1976 employment in the mining sector.

Appendix Figure A.15 shows that there is no evidence of a relationship between the evolution of infant mortality from 1950-1979 and the severity of the 1980-1982 recession. The point estimates are centered around zero, generally small in magnitude, and indistinguishable from zero ($p = 0.89$). When including counties with a high mining employment share, there is also no evidence

regression on individuals under age 18 and cluster standard errors by birth state. I use maternal migration for children born after 1975. On average, 13.7 percent of children move across CZs from 1975-1980. OLS estimates imply that a 10 percent decrease in earnings per capita is associated with a 0.8 (1.3) percentage point increase in the probability of moving across CZs.

[36] I use the same covariates and sample to estimate this regression. On average, 18 percent of my sample lives outside their birth state. OLS estimates imply that a 10 percent decrease in earnings per capita is associated with a 0.9 (3.8) percentage point increase in the probability of living outside one's birth state.

[37] Results are not sensitive to controlling for the 1950-1970 change in income.

of a significant relationship ($p = 0.67$).

Information on parental characteristics and infant birth weight are not available for the full 1950-1979 period, but are available from 1970-1979. To examine these outcomes, I estimate similar regressions, normalizing the interaction between the severity of the recession and birth year to equal 0 for individuals born in 1970. The control variables and sample are the same.

Appendix Table A.13 provides no evidence of a relationship between the evolution of maternal education or infant birth weight and the severity of the 1980-1982 recession. I examine five dependent variables: average mothers' years of schooling, the share of births classified as low birth weight (no more than 2,500 grams), very low birth weight (1,500 grams), and extremely low birth weight (1,000 grams), and median birth weight.[38] For each dependent variable, the coefficients are small and individually and jointly indistinguishable from zero.

A.8 Separating the Long-Run Effects of Temporary and Persistent Earnings Decreases on Education

My baseline specification measures recession severity using the 1978-1982 decrease in log real earnings per capita, and uses the predicted log employment change from 1978-1982 as an instrumental variable. Counties with a larger predicted employment decrease experienced a persistent decrease in local economic activity, as described in Section 1.2, and my baseline specification implicitly reflects this persistence.

Evidence on whether the long-run effects of the recession stem from temporary or persistent declines in local economic activity could shed light on the underlying mechanisms and the type of economic shock that might lead to long-run effects. For young children, a temporary decrease in economic activity could have negative long-run effects if the human capital production function features sufficiently strong dynamic complementarity or early childhood is a sensitive period of development.[39] Even in the absence of these features of childhood development, a persistent

[38] There are fewer observations for average mother's years of schooling because 13 states did not report education during part of the 1970-1977 period. All states reported education in 1978 and 1979. The state-year fixed effects in the regression control for changes in a state's reporting status over time.

[39] Dynamic complementarity implies that less investment in one period reduces the return to investment in later

decrease in economic activity could have negative long-run effects by reducing the sequence of investments in childhood human capital or parental resources to pay for college. For adolescents, a temporary or persistent decrease could reduce parental resources to pay for college.

To examine this, I estimate regressions that include the decrease in log real earnings per capita from 1978-1982 and from 1978-1992.[40] As instrumental variables, I use the predicted log employment change from 1978-1982 and 1978-1992, based on a county's 1976 industrial structure. The identification comes from the interaction of a county's pre-recession industrial specialization with aggregate employment changes from 1978-1982 and 1978-1992.[41] While this approach separates the temporary and persistent declines in earnings per capita that emerged at the onset of the 1980-1982 recession, a limitation that should be considered in interpreting these results is that not all of the industry-level employment changes from 1978-1992 are due to the 1980-1982 recession.

The point estimates in Appendix Table A.17 suggest that the negative long-run effects on four-year degree attainment arise from the persistent decline in log earnings per capita, but there is little evidence of this for any college degree attainment, and the standard errors prevent sharper conclusions.[42]

A.9 Long-Run Effects of the Recession on Education: Robustness Checks

This section summarizes results that demonstrate the robustness of my estimates to different specifications. Given its importance, I focus on the effect of the recession on four-year college degree attainment.

periods (Cunha and Heckman, 2007; Cunha, Heckman and Schennach, 2010; Aizer and Cunha, 2012; Caucutt and Lochner, 2012).

[40]As discussed in Appendix A.3, the recession displays a similar degree of persistence for years 1987-2002, so the choice of 1992 is probably not important.

[41]The 1978-1982 and 1978-1992 predicted log employment changes are highly, but not perfectly, correlated (see Appendix Figure A.20). Among all counties, state fixed effects and the 1978-1982 predicted log employment change explain 45 percent of the variation in the 1978-1992 predicted change. Appendix Table A.16 describes industry-level employment changes from 1978-1992. Comparing this with Table 1.1 reveals the patterns that distinguish the temporary and persistent effects. For example, oil and gas extraction did relatively well from 1978-1982, but poorly from 1978-1992. Auto dealers experienced large employment losses from 1978-1982, but gains from 1978-1992. Primary metal manufacturing experienced employment losses over both horizons.

[42]In the future, I will report p-values from the test of whether the effects of temporary and persistent earnings decreases are equal. This requires submitting an additional disclosure request to the Census Bureau.

Appendix Table A.18 shows that results are similar when replacing fixed effects for age in 1979 by birth state with age by birth division or region.[43] Appendix Table A.19 shows that results are robust to not controlling for interactions between age in 1979 and the 1950-1970 change in log median family income in individuals' birth county, to controlling instead for the 1950-1980 change in log median family income, and to controlling for both the 1950-1970 and 1970-1980 change.

Appendix Table A.20 shows that results are similar when replacing the 1978-1982 decrease in log earnings per capita with other measures of recession severity: the decrease in log earnings, the decrease in log income per capita, the decrease in log employment, and the decrease in earnings per capita.[44] Appendix Table A.21 shows that results are similar when using all other states in the continental U.S., instead of other states in the same region, to construct the predicted log employment change instrumental variable; estimates are similar but less precise when using the predicted log employment change in manufacturing, which was the largest industry in 1978 and experienced a severe decline in the 1980-1982 recession.

Appendix Table A.22 presents results that measure the change in log earnings per capita and the predicted log employment change at different units of geography. My main specification measures recession severity at the county-level. I also estimate regressions that measure recession severity at the commuting zone (CZ)-level. In interpreting these two sets of results, an important issue is whether the nature of the recession differs at the county or CZ-level. To examine this, I re-estimate equation (1.2), where the dependent variable is the log real median family income in a county, using the change in log earnings per capita from 1978-1982 in each county's CZ as the key explanatory variable. I also construct the predicted log employment change at the CZ-level. The results, in Appendix Figure A.7, differ somewhat from the results in Figure 1.4, where the change in log earnings per capita and predicted log employment change are measured at the county-level. When measuring recession severity at the CZ-level, there is a slight decline in log median family

[43]There are nine divisions and four regions, as defined by the Census Bureau.

[44]Mean real earnings per capita in 1978 is \$21,964, so a 10 percent decrease in earnings per capita at the mean amounts to \$2,196. The estimates using the decrease in earnings per capita in Appendix Table A.20 imply that a \$2,196 decrease in earnings per capita leads to a 3.5 percentage point ($= 0.159 \times 0.2196$) decrease in four-year degree attainment for 0-10 year olds and a 1.9 percentage point decrease for 11-19 year olds. These estimates are similar to those which use the change in log real earnings per capita, which imply a 3.0 and 1.6 percentage point decrease.

income from 1970-1980 in counties whose CZ experienced a more severe recession (in contrast, Figure 1.4 shows no change in log median family income from 1970-1980 in counties where the recession was more severe). In addition, the decline in log median family income in 1990 is smaller in magnitude than the decline in 2000 (in contrast, Figure 1.4 shows a similar decline in log median family income for 1990 and 2000, and this decline is similar to the 2000 decline in Appendix Figure A.7). In sum, the nature of the recession differs somewhat at the county and CZ-level. These results suggest that controlling for the 1970-1980 change in log median family income and separating the temporary and persistent effects of the recession could be important when comparing specifications that measure recession severity at the county versus CZ-level.

Column 1 of Appendix Table A.22 presents the baseline effects on four-year college degree attainment, where recession severity is measured at the county-level. In column 2, I separate the effects of the temporary and persistent declines in earnings per capita, as described in Appendix A.8. Columns 3 and 4 present results when measuring the severity of the recession at the CZ-level, without making any other changes to the specification. In both columns, the effects are small and indistinguishable from zero. Columns 5-8 add interactions between individuals' age in 1979 and the 1970-1980 change in log median family income in their county of birth. Columns 5-6, which measure recession severity at the county-level, are similar to columns 1-2, as expected given the lack of a 1970-1980 pre-trend in log median family income seen in Figure 1.4. Columns 7-8 measure recession severity at the CZ-level. Column 7, which does not separate the temporary and persistent declines in earnings per capita, again reveals small and indistinguishable effects. However, when separating the temporary and persistent declines in column 8, the results are broadly consistent with those in column 6, which measure recession severity at the county-level. In particular, the decrease in log real earnings per capita from 1978-1992 (i.e., the persistent component) has a negative, statistically significant, and similarly-sized effect on four-year college degree attainment. In sum, these results indicate that after modifying the specification to account for differences in the nature of the recession at the county and CZ-level, the effects of the recession on four-year college degree attainment are broadly consistent when measuring recession severity at the county and CZ-level.

Columns 9-10 present an alternative approach to assess the robustness of my results to the unit of geography. I replace the decrease in log earnings per capita in individuals' birth county with a population and distance weighted average for counties within 100 miles, and I use a similar weighted average for the predicted log employment change.[45] This approach has the benefit of distinguishing between counties within CZs, while allowing the severity of the recession in nearby counties to influence long-run outcomes. Columns 9-10 are extremely similar to columns 1-2, which provides further support for the robustness of my results.

[45]I construct the weighted average of the decrease in log real earnings per capita as

$$\bar{R}_c^{78-82} = \sum_{j:D_{c,j} \leq 100} \frac{N_j D_{c,j}^{-1}}{\sum_{j':D_{c,j'} \leq 100} N_{j'} D_{c,j'}^{-1}} R_j^{78-82}.$$

The weight increases in N_j, the 1970 population of county j, and decreases in $D_{c,j}$, the distance in miles between counties c and j. These are desirable features because larger counties are likely more popular destinations for migrants or commuters and the cost of migrating or commuting increases in distance. I normalize $D_{c,c} = 1$.

Table A.1: Approximate Replication of Tables 3 and 4 of Feyrer, Sacerdote and Stern (2007)

	Dependent variable: Change in employment rate			
	1977-1982	1982-1987	1977-1987	1987-2004
	(1)	(2)	(3)	(4)
Panel A: Table 3 of FSS				
Shock dummy	-0.013***	0.011***	-0.002	0.000
	(0.003)	(0.003)	(0.003)	(0.003)
Observations	1,439	1,439	1,439	1,439
R^2	0.37	0.40	0.47	0.31
Panel B: Attempted replication of Table 3 of FSS				
Shock dummy	-0.013***	0.012***	-0.001	-0.000
	(0.004)	(0.003)	(0.003)	(0.002)
Observations	2,326	2,326	2,326	2,326
R^2	0.28	0.32	0.38	0.24
Panel C: Table 4 of FSS				
Shock size	0.163***	-0.144***	0.019	0.020
	(0.051)	(0.052)	(0.047)	(0.039)
Observations	1,439	1,439	1,439	1,439
R^2	0.37	0.40	0.47	0.31
Panel D: Attempted replication of Table 4 of FSS				
Shock size	0.173***	-0.153***	0.020	0.019
	(0.058)	(0.058)	(0.048)	(0.036)
Observations	2,326	2,326	2,326	2,326
R^2	0.28	0.31	0.38	0.24

Notes: The dependent variable is 1 minus the unemployment rate, which FSS and I refer to as the employment rate. Shock size is the 1977-1982 employment change in the auto and steel industries divided by 1977 total employment. Shock dummy equals one if shock size is less than or equal to -0.02 (i.e., at least two percent of employment lost). All regressions include Census division indicators and an indicator for whether a county is in an MSA in 2000. Sample limited to counties with at least 10,000 residents in 1977. Heteroskedasticity robust standard errors in parentheses.
Sources: Panels A and C are from Tables 3 and 4 of Feyrer, Sacerdote and Stern (2007). Panels B and D are from BLS Local Area Statistics, Census County Business Patterns, and Census Annual Population Estimates

Table A.2: Comparison to Feyrer, Sacerdote and Stern (2007): Results from Different Dependent Variables with FSS Specification

	Dependent variable: Log change in				
	Employment rate	Employment-pop. 15+ ratio	Employment-pop. 15+ ratio	Employment-pop. ratio	Earnings per capita
	(1)	(2)	(3)	(4)	(5)
Panel A: Dependent variable is log change from 1977-1982					
Shock size	0.201***	0.485***	0.622***	0.647***	0.659***
	(0.0674)	(0.150)	(0.118)	(0.121)	(0.124)
R^2	0.267	0.061	0.111	0.107	0.248
Panel B: Dependent variable is log change from 1977-1982					
Shock size	0.194**	0.542***	0.417***	0.408***	0.414***
using estabs.	(0.0969)	(0.150)	(0.126)	(0.125)	(0.131)
R^2	0.280	0.075	0.122	0.117	0.255
Panel C: Dependent variable is log change from 1977-1987					
Shock size	0.0185	0.378	0.572***	0.575***	0.787***
	(0.0530)	(0.235)	(0.176)	(0.178)	(0.170)
R^2	0.368	0.071	0.105	0.134	0.268
Panel D: Dependent variable is log change from 1977-1987					
Shock size	-0.0287	0.0936	0.141	0.107	0.224
using estabs.	(0.0702)	(0.250)	(0.224)	(0.215)	(0.182)
R^2	0.368	0.071	0.102	0.132	0.265
Source of employment data:	BLS	BLS	BEA	BEA	N/A
Observations	2,326	2,326	2,326	2,326	2,326

Notes: The employment rate is 1 minus the unemployment rate. Shock size is the 1977-1982 employment change in the auto and steel industries divided by 1977 total employment. As defined by FSS, the employment change comes from CBP employment counts, which are frequently suppressed. Shock size using establishments uses CBP establishment counts, which are never suppressed. See text for details. All regressions include Census division indicators and an indicator for whether the county is in an MSA in 2000. Sample limited to counties with at least 10,000 residents in 1977. Heteroskedasticity robust standard errors in parentheses.

Sources: BLS Local Area Statistics, Census County Business Patterns, and Census Annual Population Estimates

Table A.3: Comparison to Feyrer, Sacerdote and Stern (2007): Results from Different Shock Measures and Different Samples

	Dependent variable: 1977-1987 log change in		
	Employment rate	Employment-pop. 15+ ratio	Earnings per capita
	(1)	(2)	(3)
Panel A: Counties with at least 10,000 residents in 1977, OLS ($N = 2,326$)			
Shock size	-0.0261	0.419**	0.647***
	(0.0723)	(0.182)	(0.237)
Panel B: Counties with at least 10,000 residents in 1977, OLS ($N = 2,326$)			
Shock size using estabs.	-0.0303	0.136	0.219
	(0.0708)	(0.241)	(0.197)
Panel C: All counties, OLS ($N = 3,076$)			
Shock size using estabs.	-0.0274	0.131	0.187
	(0.0684)	(0.219)	(0.177)
Panel D: All counties, OLS ($N = 3,076$)			
Change in log earnings per capita, 1978-1982	0.00891	0.295***	0.399***
	(0.00884)	(0.0439)	(0.0568)
Panel E: All counties, 2SLS, all industries ($N = 3,076$)			
Change in log earnings per capita, 1978-1982	-0.0969	0.370**	0.102
	(0.0689)	(0.169)	(0.234)
Panel F: Low mining counties, OLS ($N = 2,550$)			
Shock size using estabs.	-0.0513	0.0903	0.134
	(0.0688)	(0.233)	(0.187)
Panel G: Low mining counties, OLS ($N = 2,550$)			
Change in log earnings per capita, 1978-1982	0.0253**	0.357***	0.478***
	(0.0102)	(0.0591)	(0.0617)
Panel H: Low mining counties, 2SLS, all industries ($N = 2,550$)			
Change in log earnings per capita, 1978-1982	0.130**	1.111***	0.962***
	(0.0542)	(0.212)	(0.184)
Panel I: Low mining counties, 2SLS, shock size ($N = 2,550$)			
Change in log earnings per capita, 1978-1982	-0.190	0.335	0.496
	(0.312)	(0.750)	(0.530)
Source of employment data:	BLS	BEA	N/A

Notes: The employment rate is 1 minus the unemployment rate. Shock size is the 1977-1982 employment change in the auto and steel industries divided by 1977 total employment. As defined by FSS, the employment change comes from CBP employment counts, which are frequently suppressed. Shock size using establishments uses CBP establishment counts, which are never suppressed. All regressions include Census division indicators. Low mining counties have less than 5 percent of 1976 employment in the mining sector. Panels E and H use the predicted log employment change in all industries from 1978-1982 as an IV. Panel I uses the FSS shock size using establishments as an IV. Standard errors clustered by state in parentheses.
Sources: BLS Local Area Statistics, Census County Business Patterns, Census Annual Population Estimates, and BEA Regional Economic Accounts data

Table A.4: The Persistence of the 1980-1982 Recession for Earnings per Capita, OLS and 2SLS Estimates

	OLS	2SLS	2SLS	2SLS
Instrument:	-	All Industries	All Industries	Manufacturing
	(1)	(2)	(3)	(4)
Panel A: OLS and 2SLS estimates (dependent variable: log earnings per capita, 1992)				
Log earnings per capita, 1982 ($\hat{\beta}$)	0.636***	1.318***	0.447*	1.157***
	(0.0574)	(0.119)	(0.254)	(0.105)
Log earnings per capita, 1978 ($\hat{\gamma}$)	0.289***	-0.348***	0.469*	-0.206*
	(0.0558)	(0.112)	(0.241)	(0.107)
$\hat{\beta}+\hat{\gamma}$	0.925***	0.971***	0.916***	0.951***
	(0.020)	(0.020)	(0.025)	(0.018)
Panel B: First stage estimates (dependent variable: log earnings per capita, 1982)				
Predicted log employment change, 1978-1982		0.412***	0.386***	0.537***
		(0.0666)	(0.0679)	(0.108)
F-statistic, slope coefficient equals 0		38.19	32.42	24.73
Exclude high mining counties	No	Yes	No	No
Observations	3,076	2,550	3,076	3,076

Notes: Panel A reports OLS and 2SLS estimates of equation (A.7), where the dependent variable is log real earnings per capita in 1992. Panel B reports the associated first stage coefficient on the predicted log employment change, where the dependent variable is log real earnings per capita in 1982. All regressions include state fixed effects and control for log real earnings per capita in 1978 and the 1950-1970 change in log real median family income. High mining counties have at least 5 percent of 1976 employment in the mining sector. Standard errors in parentheses are clustered by state.

Sources: BEA Regional Economic Accounts, County Business Patterns, Census County Data Books, Minnesota Population Center (2011)

Table A.5: The Persistence of the 1980-1982 Recession for Employment-Population Ratio, OLS and 2SLS Estimates

	OLS	2SLS	2SLS	2SLS
Instrument:	-	All industries	All industries	Manufacturing
	(1)	(2)	(3)	(4)
Panel A: OLS and 2SLS estimates (dependent variable: log employment-population ratio, 1992)				
Log employment-pop. ratio, 1982 ($\hat{\beta}$)	0.610***	1.430***	0.369	1.006***
	(0.118)	(0.156)	(0.235)	(0.184)
Log employment-pop. ratio, 1978 ($\hat{\gamma}$)	0.288**	-0.491***	0.521**	-0.0949
	(0.109)	(0.153)	(0.223)	(0.180)
$\hat{\beta} + \hat{\gamma}$	0.898***	0.939***	0.890***	0.911***
	(0.017)	(0.014)	(0.020)	(0.016)
Panel B: First stage estimates (dependent variable: log employment-population ratio, 1982)				
Predicted log employment change, 1978-82		0.355***	0.375***	0.458***
		(0.0456)	(0.0395)	(0.0668)
F-statistic, slope coefficient equals 0		60.39	90.13	46.96
Exclude high mining counties	No	Yes	No	No
Observations	3,076	2,550	3,076	3,076

Notes: Panel A reports OLS and 2SLS estimates of equation (A.7), where the dependent variable is the log employment-population ratio in 1992. Panel B reports the associated first stage coefficient on the predicted log employment change, where the dependent variable is the log employment-population ratio in 1982. See notes to Appendix Table A.4.

Sources: BEA Regional Economic Accounts, County Business Patterns, Census County Data Books, Minnesota Population Center (2011)

Table A.6: The Persistence of the 1980-1982 Recession, OLS and 2SLS Estimates, At Different Horizons

Persistence Horizon:	1987 (1)	1992 (2)	1997 (3)	2002 (4)	2007 (5)	2012 (6)
Panel A: OLS and 2SLS estimates (dependent variable: log earnings per capita in indicated year)						
Log earnings per capita, 1982 ($\hat{\beta}$)	1.236***	1.318***	1.310***	1.742***	2.251***	2.556***
	(0.122)	(0.119)	(0.134)	(0.204)	(0.249)	(0.294)
Log earnings per capita, 1978 ($\hat{\gamma}$)	-0.250**	-0.348***	-0.330***	-0.748***	-1.207***	-1.576***
	(0.118)	(0.112)	(0.127)	(0.203)	(0.248)	(0.294)
$\hat{\beta}+\hat{\gamma}$	0.986***	0.971***	0.980***	0.994***	1.044***	0.981***
	(0.014)	(0.020)	(0.025)	(0.023)	(0.027)	(0.035)
Observations	2,550	2,550	2,550	2,550	2,550	2,550
Panel B: OLS and 2SLS estimates (dependent variable: log employment-population ratio in indicated year)						
Log employment-pop. ratio, 1982 ($\hat{\beta}$)	1.269***	1.430***	1.745***	2.659***	3.031***	3.238***
	(0.136)	(0.156)	(0.249)	(0.339)	(0.394)	(0.372)
Log employment-pop. ratio, 1978 ($\hat{\gamma}$)	-0.317**	-0.491***	-0.843***	-1.759***	-2.147***	-2.374***
	(0.136)	(0.153)	(0.239)	(0.327)	(0.382)	(0.366)
$\hat{\beta}+\hat{\gamma}$	0.951***	0.939***	0.902***	0.900***	0.885***	0.864***
	(0.011)	(0.014)	(0.022)	(0.034)	(0.038)	(0.042)
Observations	2,550	2,550	2,550	2,550	2,550	2,550

Notes: Panel A reports OLS and 2SLS estimates of equation (A.7), where the dependent variable is log real earnings per capita in the indicated year. In Panel B, the dependent variable is the log employment-population ratio in the indicated year. I use the predicted log employment change in all industries as the IV and exclude counties with at least 5 percent employment in the mining sector in 1976. All regressions include state fixed effects and control for log real earnings per capita in 1978 and the 1950-1970 change in log real median family income. Standard errors in parentheses are clustered by state.

Sources: BEA Regional Economic Accounts, County Business Patterns, Census County Data Books, Minnesota Population Center (2011)

Table A.7: The Effect of the 1980-1982 Recession on Log Median Family Income, Rents, and House Values, 2SLS Estimates

	Dependent variable: 1980-1990 change in		
	Log median family income	Log median rent	Log median house value
	(1)	(2)	(3)
Panel A: Excluding high mining counties			
Change in log real earnings	1.004***	0.721***	0.780**
per capita, 1978-1982	(0.166)	(0.246)	(0.349)
Observations	2,550	2,550	2,550
Panel B: All counties			
Change in log real earnings	0.221	0.003	0.220
per capita, 1978-1982	(0.182)	(0.156)	(0.244)
Observations	3,076	3,076	3,076

Notes: I use the predicted log employment change in all industries as the IV. Regressions include state fixed effects and the change in log real median family income from 1950-1970. High mining counties have at least 5 percent of 1976 employment in the mining sector. Standard errors in parentheses are clustered by state.
Sources: BEA Regional Economic Accounts, Census County Business Patterns, Census County Data Books, Minnesota Population Center (2011)

Table A.8: The Effects of the 1980-1982 Recession on Local Government Expenditures, 2SLS Estimates

	Dependent variable: Log expenditure							
		By purpose					By type	
	General direct expenditures	Education	Public safety	Welfare and health	Infra-structure	Other	Current	Capital
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Interaction between 1978-1982 decrease in log real earnings per capita and year								
1972	-0.0398	-0.450	-0.0823	-0.270	-0.0145	-0.0320	0.181	-1.158
	(0.275)	(0.519)	(0.575)	(1.425)	(0.558)	(0.568)	(0.273)	(1.219)
1982	0.117	0.200	1.069**	-0.624	0.420	-0.0687	0.0849	-0.363
	(0.226)	(0.239)	(0.435)	(1.410)	(0.635)	(0.441)	(0.189)	(1.240)
1987	-0.245	0.212	0.494	0.0258	-0.709	-0.549	-0.153	-1.770
	(0.245)	(0.227)	(0.552)	(1.461)	(0.830)	(0.564)	(0.199)	(1.341)
1992	-1.123***	-0.145	0.0639	-5.317***	-0.812	-1.751***	-1.021***	-2.299**
	(0.308)	(0.284)	(0.593)	(1.813)	(0.844)	(0.625)	(0.313)	(1.080)
1997	-0.878***	-0.168	0.604	-3.071**	-0.861	-1.887***	-0.812***	-1.484
	(0.318)	(0.325)	(0.511)	(1.416)	(0.793)	(0.661)	(0.296)	(1.184)
Observations	15,270	15,270	15,270	15,270	15,270	15,270	15,270	15,270
Real per capita mean, 1977	$2,444	$1,287	$137	$293	$328	$400	$2,109	$335
Share of total, 1977	1.000	0.527	0.056	0.120	0.134	0.164	0.863	0.137

Notes: The interaction between the 1978-1982 decrease in log real earnings per capita and year 1977 is normalized to equal 0. Regressions are estimated by 2SLS, using the predicted log employment change in all industries from 1978-1982 as an IV. Regressions include fixed effects for county and state-by-year, interactions between year and the 1950-1970 change in log median family income, log population, and the share of the population which is age 0-4, 5-19, and 20-64. I transform dependent variables using the inverse hyperbolic sine instead of the log because a small number of observations equal zero. Sample limited to counties with no more than 5 percent of 1976 employment in the mining sector, and sample excludes 5 counties in New York City. Standard errors in parentheses are clustered by state.

Sources: Census of Governments, BEA Regional Economic Accounts , Census County Business Patterns, Census County Data Books, Minnesota Population Center (2011)

Table A.9: The Effects of the 1980-1982 Recession on Local Government Revenues, 2SLS Estimates

	Dependent variable: Log revenue							
		By broad source			By selected detailed source			
	General direct revenue	Taxes	Charges	Intergov't transfers	Property taxes	Federal transfers	State transfers	Local transfers
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Interaction between 1978-1982 decrease in log real earnings per capita and year								
1972	0.206	0.552	0.766	0.244	0.750*	-3.204	-0.419	-0.0115
	(0.294)	(0.374)	(0.828)	(0.342)	(0.443)	(3.306)	(0.367)	(2.397)
1982	-0.132	-0.581**	0.290	0.167	-0.482	-0.0531	-0.0775	1.683
	(0.225)	(0.276)	(0.589)	(0.280)	(0.295)	(0.921)	(0.236)	(1.593)
1987	-0.304	-1.101**	-0.353	0.499	-0.927*	1.833	-0.450	2.162
	(0.262)	(0.493)	(0.633)	(0.503)	(0.522)	(1.411)	(0.564)	(2.728)
1992	-0.964***	-1.493***	-1.756***	-0.0838	-1.748***	-0.921	-0.994*	0.916
	(0.284)	(0.559)	(0.654)	(0.482)	(0.597)	(2.220)	(0.524)	(2.924)
1997	-0.654**	-0.448	-1.310*	-0.313	-0.467	1.607	-1.281**	-0.764
	(0.290)	(0.477)	(0.782)	(0.490)	(0.419)	(1.737)	(0.612)	(2.489)
Observations	15,270	15,270	15,270	15,270	15,270	15,270	15,270	15,270
Real per capita mean, 1977	$2,566	$943	$437	$1,186	$840	$182	$934	$70
Share of total, 1977	1.000	0.367	0.170	0.462	0.327	0.071	0.364	0.027

Notes: See notes to Appendix Table A.8.

Sources: Census of Governments, BEA Regional Economic Accounts , Census County Business Patterns, Census County Data Books, Minnesota Population Center (2011)

Table A.10: Sample Construction and Match Statistics

Panel A: Basic sample construction	
Individuals who meet baseline demographic criteria	27,374,000
Individuals with non-missing PIK	26,253,000
Individuals with non-duplicate PIK	26,147,000
Individuals with unique birth county	24,462,000
Panel B: Birth county match type, as share of total	
Exact	0.7685
Exact - abbreviation	0.0357
Duplicate	0.1095
Probabilistic	0.0511
Hand check	0.0352

Notes: The baseline demographic criteria are having non-imputed values for state of birth, birth year, sex, and race, plus being born in the U.S. according to the Census/ACS survey. A duplicate PIK is one which appears more than once in survey year. Sample contains individuals born from 1950-1980.
Source: Confidential 2000-2013 Census/ACS data linked to the SSA NUMIDENT file

Table A.11: Correlation of County-Level Shocks Across Recessions

	Log earnings per capita change 1973-75 (1)	Log earnings per capita change 1978-82 (2)	Log earnings per capita change 1989-91 (3)	Log earnings per capita change 2000-02 (4)	Log earnings per capita change 2007-10 (5)	Predicted log employment change 1978-82 (6)
Panel A: Raw correlations						
Log earnings per capita change, 1973-75	1.000					
Log earnings per capita change,1978-82	-0.027	1.000				
Log earnings per capita change,1989-91	-0.023	0.050	1.000			
Log earnings per capita change,2000-02	0.132	0.117	-0.010	1.000		
Log earnings per capita change,2007-10	-0.171	-0.013	0.107	-0.104	1.000	
Predicted log employment change, 1978-82	0.036	0.366	0.201	0.149	0.025	1.000
Panel B: Conditional on state fixed effects						
Log earnings per capita change, 1973-75	1.000					
Log earnings per capita change,1978-82	-0.064	1.000				
Log earnings per capita change,1989-91	0.026	0.022	1.000			
Log earnings per capita change,2000-02	0.077	0.063	0.004	1.000		
Log earnings per capita change,2007-10	-0.072	-0.056	0.013	-0.090	1.000	
Predicted log employment change, 1978-82	-0.061	0.212	0.088	0.033	0.060	1.000

Notes: The predicted log employment change from 1978-82 is constructed using a county's 1976 industrial structure and the industry-level log employment change from 1978-1982 in other states within the same region, as defined in equation (1.1). Sample contains 3,076 counties in the continental U.S.
Sources: BEA Regional Economic Accounts, County Business Patterns

Table A.12: Stability of the Relationship between Severity of 1980-1982 Recession in County of Residence and County of Birth Across Cohorts

	Dependent variable: 1978-1982 decrease in log real earnings per capita in county of residence in year			
	1979	1991	2003	2013
	(1)	(2)	(3)	(4)
Interaction between 1978-1982 decrease in log real earnings per capita in county of birth and age				
0-1	0.969***	0.973***	1***	1***
	(0.0277)	(0.0214)	(0.000)	(0.000)
2-4	0.854***	0.878***	0.919***	0.856***
	(0.0321)	(0.0262)	(0.0314)	(0.0266)
5-7	0.803***	0.803***	0.778***	0.807***
	(0.0499)	(0.0580)	(0.0481)	(0.0313)
8-10	0.747***	0.621***	0.715***	0.811***
	(0.0468)	(0.0606)	(0.0774)	(0.0415)
11-13	0.715***	0.704***	0.667***	
	(0.0844)	(0.118)	(0.0713)	
Observations	3,684	4,028	3,336	3,358
p-value, coefficients equal to column 1	-	0.355	0.273	0.713
Sample: individuals born in years	1968-1979	1980-1991	1992-2003	2004-2013

Notes: Table reports estimates of OLS regressions. The dependent variable is the 1978-1982 decrease in log real earnings per capita in individuals' county of residence in the indicated year. Regressions include fixed effects for birth year-by-birth state and birth year interacted with the 1950-1970 change in log median family income in individuals' county of birth. The coefficients in column 1 are plotted in Appendix Figure A.11.
Sources: BEA Regional Economic Accounts, Confidential PSID data

Table A.13: Maternal Education and Infant Health Did Not Evolve Differentially Before the 1980-1982 Recession

	Dependent variable:				
	Average mothers' years of schooling (1)	Share low birth weight (2)	Share very low birth weight (3)	Share extremely low birth weight (4)	Median birth weight, grams (5)
Interaction between 1978-1982 decrease in log real earnings per capita and birth year					
1971-1973	0.476	0.0096	-0.0149	-0.0030	252.4
	(0.391)	(0.0562)	(0.0161)	(0.0128)	(174.3)
1974-1976	0.518	0.0180	-0.0042	0.0088	94.54
	(0.392)	(0.0433)	(0.0123)	(0.0123)	(113.1)
1977-1979	0.0459	-0.0005	0.0022	0.0128	84.54
	(0.448)	(0.0423)	(0.0145)	(0.0116)	(125.3)
Observations	21,084	25,497	25,497	25,497	25,497
p-value, all coefs. equal 0	0.416	0.884	0.782	0.131	0.341
Dep. var. mean, 1970-1979	11.87	0.0698	0.0103	0.0040	3,356

Notes: The interaction between the 1978-1982 decrease in log real earnings per capita and birth year 1970 is normalized to equal zero. Regressions are estimated by 2SLS, using the predicted log employment change in all industries from 1978-1982 as an IV. Regressions include fixed effects for county and state-by-year, plus interactions between year and the 1950-1970 change in log median family income. Standard errors in parentheses are clustered by state. Low birth weight is defined as no more than 2,500 grams, very low birth weight is no more than 1,500 grams, and extremely low birth weight is no more than 1,000 grams.

Sources: National Center for Health Statistics (1970-1979), BEA Regional Economic Accounts, County Business Patterns, Census County Data Books, Minnesota Population Center (2011)

Table A.14: The Long-Run Effects of the 1980-1982 Recession on Educational Attainment, OLS and Reduced-Form Estimates

	Dependent variable:					
	HS/GED attainment	Any college attendance	Any college degree attainment	Four-year college degree attainment	Two-year college degree attainment	Years of schooling
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: OLS estimates						
Interaction between 1978-1982 decrease in log real earnings per capita and age in 1979						
0-10	0.0295**	0.0345	-0.0104	-0.0521*	0.0417**	0.0686
	(0.0130)	(0.0260)	(0.0296)	(0.0278)	(0.0187)	(0.166)
11-19	0.0242***	0.0519**	0.0309	0.0159	0.0150	0.279**
	(0.0089)	(0.0224)	(0.0212)	(0.0182)	(0.0141)	(0.119)
20-28	0.0149**	0.0268*	0.0311**	0.0308**	0.0003	0.234***
	(0.0060)	(0.0155)	(0.0133)	(0.0136)	(0.0102)	(0.0748)
Panel B: Reduced-form estimates						
Interaction between 1978-1982 predicted log employment decrease and age in 1979						
0-10	0.0198	-0.0154	-0.0987***	-0.163***	0.0648**	-0.242
	(0.0203)	(0.0264)	(0.0305)	(0.0427)	(0.0245)	(0.184)
11-19	0.0186	-0.0491	-0.0610**	-0.0785**	0.0175	-0.0462
	(0.0150)	(0.0310)	(0.0280)	(0.0330)	(0.0216)	(0.152)
20-28	0.0078	-0.0253	0.0156	0.0190	-0.0033	0.182*
	(0.0128)	(0.0231)	(0.0170)	(0.0210)	(0.0161)	(0.100)

Notes: Panel A reports estimates of the interaction between the 1978-1982 decrease in log real earnings per capita in individuals' county of birth and indicators for age in 1979. Panel B reports estimates of the interaction between the 1978-1982 predicted log employment decrease in individuals' county of birth and indicators for age in 1979. See notes to Table 1.2.

Sources: BEA Regional Economic Accounts, Census County Business Patterns, Confidential 2000-2013 Census/ACS data linked to the SSA NUMIDENT file

Table A.15: The Long-Run Effects of the 1980-1982 Recession on Educational Attainment, First Stage Estimates

	Dependent variable: Interaction between 1978-1982 decrease in log real earnings per capita and age in 1979		
	20-28	11-19	0-10
	(1)	(2)	(3)
Interaction between 1978-1982 predicted log employment decrease and age in 1979			
0-10	-0.0397***	-0.0336***	0.552***
	(0.00874)	(0.00791)	(0.0761)
11-19	-0.0251***	0.516***	-0.0138***
	(0.00722)	(0.0744)	(0.00458)
20-28	0.494***	-0.0110***	-0.00699***
	(0.0714)	(0.00363)	(0.00250)
F-statistic, all coefficients equal 0	20.20	18.13	19.55

Notes: Table reports first stage estimates of the 2SLS system. See notes to Table 1.2.
Sources: BEA Regional Economic Accounts, Census County Business Patterns, Confidential 2000-2013 Census/ACS data linked to the SSA NUMIDENT file

Table A.16: Aggregate Employment Changes from 1978-1992, by Industry

	Share of total 1978 employment (1)	Log employment change (2)	Employment change (3)
Panel A: Overall and one-digit industries			
All industries	1.000	0.317	25,861,062
Manufacturing	0.289	-0.105	-2,007,089
Mining	0.012	-0.223	-164,018
Agriculture, forestry, and fisheries	0.004	0.878	378,609
Construction	0.058	0.167	732,219
Transportation and public utilities	0.062	0.257	1,258,595
Wholesale trade	0.070	0.262	1,464,533
Finance, insurance, and real estate	0.070	0.379	2,251,947
Retail trade	0.206	0.356	6,115,470
Services	0.221	0.717	16,066,416
Panel B: Two-digit industries with largest employment decrease			
Primary metal (manufacturing)	0.017	-0.507	-439,712
Industrial machinery (manufacturing)	0.033	-0.191	-383,492
Electronic equipment (manufacturing)	0.027	-0.236	-383,070
Apparel and other textile products (manufacturing)	0.019	-0.255	-289,581
Textile mill products (manufacturing)	0.013	-0.321	-235,813
Transportation equipment (manufacturing)	0.025	-0.145	-220,057
Fabricated metal products (manufacturing)	0.024	-0.140	-210,290
Stone, clay, and glass products (manufacturing)	0.010	-0.276	-154,170
Leather (manufacturing)	0.004	-0.827	-134,162
Heavy construction (construction)	0.011	-0.111	-76,597
Panel C: Two-digit industries with largest employment increase			
Durables (wholesale trade)	0.041	0.251	782,035
Miscellaneous retail (retail trade)	0.027	0.378	829,284
Depository institutions (finance)	0.021	0.481	841,251
Membership organizations (services)	0.019	0.526	861,985
Food stores (retail trade)	0.031	0.446	1,146,502
Social services (services)	0.013	0.831	1,147,737
Miscellaneous services (services)	0.011	1.324	2,071,041
Eating and drinking places (retail trade)	0.060	0.534	2,829,410
Business services (services)	0.038	0.771	2,966,886
Health services (services)	0.070	0.752	5,226,976

Notes: I construct this table by aggregating county-level data for the continental United States. Because employment is often suppressed at the county-level, I impute employment using the number of establishments and nationwide information on employment by establishment size, as described in Appendix A.1.
Source: Census County Business Patterns

Table A.17: The Long-Run Effects of the 1980-1982 Recession on Educational Attainment, Separating the Temporary and Persistent Decline in Log Earnings per Capita

	Dependent variable:					
	HS/GED attainment	Any college attendance	Any college degree attainment	Four-year college degree attainment	Two-year college degree attainment	Years of schooling
	(1)	(2)	(3)	(4)	(5)	(6)
Interaction between 1978-1982 decrease in log real earnings per capita and age in 1979						
0-10	-0.0073	0.0806	-0.118	-0.0693	-0.0485	-0.464
	(0.0455)	(0.189)	(0.0744)	(0.101)	(0.0537)	(0.467)
11-19	0.0567	0.110	-0.117**	-0.0729	-0.0443	-0.218
	(0.0363)	(0.194)	(0.0517)	(0.0585)	(0.0457)	(0.286)
20-28	0.0173	0.283	-0.0039	0.0297	-0.0336	0.0688
	(0.0296)	(0.203)	(0.0431)	(0.0453)	(0.0347)	(0.223)
Interaction between 1978-1992 decrease in log real earnings per capita and age in 1979						
0-10	0.0352	-0.0694	-0.0496	-0.175***	0.125***	0.0357
	(0.0223)	(0.102)	(0.0437)	(0.0446)	(0.0308)	(0.266)
11-19	-0.0136	-0.143	-0.0027	-0.0621**	0.0593***	0.103
	(0.0133)	(0.111)	(0.0305)	(0.0298)	(0.0151)	(0.160)
20-28	-0.0003	-0.191	0.0235	0.0031	0.0204**	0.221**
	(0.0123)	(0.117)	(0.0201)	(0.0209)	(0.0089)	(0.0946)

Notes: Table reports estimates of the interaction between the 1978-1982 and 1978-1992 decrease in log real earnings per capita in individuals' birth county and indicators for age in 1979. Regressions are estimated by 2SLS, using the predicted log employment change in all industries from 1978-1982 and 1978-1992 as instrumental variables. See notes to Table 1.2.

Sources: BEA Regional Economic Accounts, Census County Business Patterns, Confidential 2000-2013 Census/ACS data linked to the SSA NUMIDENT file

Table A.18: The Long-Run Effect of the 1980-1982 Recession on Four-Year College Degree Attainment, Robustness to Fixed Effects

	(1)	(2)	(3)
Interaction between 1978-1982 decrease in log real earnings per capita and age in 1979			
0-10	-0.303***	-0.244***	-0.223***
	(0.109)	(0.0830)	(0.0789)
11-19	-0.159**	-0.102*	-0.0999*
	(0.0801)	(0.0614)	(0.0599)
20-28	0.0306	0.0409	0.0407
	(0.0426)	(0.0365)	(0.0345)
Age in 1979 by birth state fixed effects	X		
Age in 1979 by birth division fixed effects		X	
Age in 1979 by birth region fixed effects			X

Notes: The dependent variable is an indicator for four-year college degree attainment. See notes to Table 1.2. There are nine divisions and four regions, as defined by the Census Bureau.
Sources: BEA Regional Economic Accounts, Census County Business Patterns, Confidential 2000-2013 Census/ACS data linked to the SSA NUMIDENT file

Table A.19: The Long-Run Effect of the 1980-1982 Recession on Four-Year College Degree Attainment, Robustness to Controlling for Pre-Recession Evolution of Family Income

	(1)	(2)	(3)	(4)
Interaction between 1978-1982 decrease in log real earnings per capita and age in 1979				
0-10	-0.241**	-0.303***	-0.316***	-0.311***
	(0.102)	(0.109)	(0.113)	(0.115)
11-19	-0.0989	-0.159**	-0.165**	-0.162*
	(0.0821)	(0.0801)	(0.0829)	(0.0830)
20-28	0.0784	0.0306	0.0257	0.0280
	(0.0487)	(0.0426)	(0.0428)	(0.0427)
Interaction between age in 1979 and change in log real median family income from				
1950-1970		X		X
1950-1980			X	
1970-1980				X

Notes: The dependent variable is an indicator for four-year college degree attainment. See notes to Table 1.2.
Sources: BEA Regional Economic Accounts, Census County Business Patterns, Confidential 2000-2013 Census/ACS data linked to the SSA NUMIDENT file

Table A.20: The Long-Run Effect of the 1980-1982 Recession on Four-Year College Degree Attainment, Robustness to Measure of Recession Severity

	Measure of recession: 1978-1982 change in				
	Log earnings per capita	Log earnings	Log income per capita	Log employment	Earnings per capita, $10k
	(1)	(2)	(3)	(4)	(5)
Interaction between measure of recession severity and age in 1979					
0-10	-0.303***	-0.249***	-0.470**	-0.220***	-0.159**
	(0.109)	(0.0850)	(0.194)	(0.0580)	(0.0686)
11-19	-0.159**	-0.129**	-0.252*	-0.113**	-0.0851*
	(0.0801)	(0.0651)	(0.141)	(0.0467)	(0.0498)
20-28	0.0306	0.0238	0.0452	0.0227	0.0143
	(0.0426)	(0.0351)	(0.0675)	(0.0315)	(0.0235)

Notes: The dependent variable is an indicator for four-year college degree attainment. Table reports estimates of the interaction between the indicated measure of recession severity in individuals' birth county and indicators for age in 1979. See notes to Table 1.2.
Sources: BEA Regional Economic Accounts, Census County Business Patterns, Confidential 2000-2013 Census/ACS data linked to the SSA NUMIDENT file

Table A.21: The Long-Run Effect of the 1980-1982 Recession on Four-Year College Degree Attainment, Robustness to Instrumental Variable

	Instrumental variable: Predicted log employment decrease from 1978-1982 in		
	All industries All regions	All industries Same region	Manufacturing Same region
	(1)	(2)	(3)
Interaction between 1978-1982 decrease in log real earnings per capita and age in 1979			
0-10	-0.389***	-0.303***	-0.278*
	(0.112)	(0.109)	(0.155)
11-19	-0.191**	-0.159**	-0.159*
	(0.0873)	(0.0801)	(0.0917)
20-28	0.00831	0.0306	0.0423
	(0.0480)	(0.0426)	(0.0477)

Notes: The dependent variable is an indicator for four-year college degree attainment. See notes to Table 1.2. See equation (1.1) for definition of instrumental variables.
Sources: BEA Regional Economic Accounts, Census County Business Patterns, Confidential 2000-2013 Census/ACS data linked to the SSA NUMIDENT file

Table A.22: The Long-Run Effect of the 1980-1982 Recession on Four-Year College Degree Attainment, Robustness to Level of Geography Used to Measure Recession Severity

	Level of geography used to measure recession severity:									
	County		CZ		County		CZ		County	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Interaction between 1978-1982 decrease in log real earnings per capita and age in 1979										
0-10	-0.303***	-0.0693	-0.0371	0.0126	-0.311***	0.0190	-0.0343	0.0753	-0.283**	-0.0529
	(0.109)	(0.101)	(0.0747)	(0.0869)	(0.115)	(0.114)	(0.0773)	(0.0957)	(0.126)	(0.144)
11-19	-0.159**	-0.0729	0.0137	0.0155	-0.162*	-0.0271	0.0161	0.0438	-0.138	-0.0771
	(0.0801)	(0.0585)	(0.0569)	(0.0674)	(0.0830)	(0.0582)	(0.0577)	(0.0615)	(0.0867)	(0.0813)
20-28	0.0306	0.0297	0.0467	0.0520	0.0280	0.0590	0.0487	0.0726	0.0368	0.0361
	(0.0426)	(0.0453)	(0.0490)	(0.0578)	(0.0427)	(0.0476)	(0.0491)	(0.0550)	(0.0541)	(0.0641)
Interaction between 1978-1992 decrease in log real earnings per capita and age in 1979										
0-10		-0.175***		-0.0755		-0.250***		-0.168***		-0.237***
		(0.0446)		(0.0582)		(0.0477)		(0.0543)		(0.0654)
11-19		-0.0621**		-0.0056		-0.0999***		-0.0491		-0.0657
		(0.0298)		(0.0392)		(0.0289)		(0.0373)		(0.0485)
20-28		0.0031		-0.0094		-0.0202		-0.0405		0.0023
		(0.0209)		(0.0282)		(0.0211)		(0.0302)		(0.0307)
Includes neighboring counties									X	X
Interaction between age in 1979 and change in log median family income from 1970-1980					X	X	X	X		

Notes: The dependent variable is an indicator for four-year college degree attainment. See notes to Table 1.2. The decrease in log real earnings per capita and the predicted log employment decrease are measured at the same level of geography. In Columns 9 and 10, the measure of the recession and instrumental variable are the weighted average among counties within 100 miles, as described in the text.
Sources: BEA Regional Economic Accounts, Census County Business Patterns, Confidential 2000-2013 Census/ACS data linked to the SSA NUMIDENT file

Table A.23: State-Level Heterogeneity

	State recession		State mean transfers		State transfer slope	
State (1)	More vs. less severe (2)	Change in log real earnings per capita, 1978-82 (3)	More vs. less generous (4)	Residual (5)	More vs. less progressive (6)	Slope (7)
AL	More	-0.081	Less	0.023	Less	-0.645
AZ	Less	-0.047	Less	-0.398	More	-1.470
AR	More	-0.105	More	0.142	More	-1.197
CA	Less	-0.048	More	0.349	More	-0.902
CO	Less	0.009	Less	0.008	Less	-0.713
CT	Less	0.026	Less	-0.030	Less	0.148
DE	Less	-0.028	Less	-0.183	Less	0.633
DC	More	-0.120	More	0.159	Less	-0.824
FL	Less	-0.017	Less	-0.741	Less	-0.563
GA	Less	-0.048	More	0.126	More	-1.071
ID	More	-0.141	Less	-0.012	Less	0.984
IL	More	-0.083	Less	-0.063	More	-1.287
IN	More	-0.136	Less	-0.522	More	-1.334
IA	More	-0.134	Less	-0.046	Less	-0.308
KS	Less	-0.001	More	0.108	Less	0.002
KY	More	-0.071	More	0.395	More	-1.339
LA	Less	0.012	More	0.364	More	-1.073
ME	Less	-0.026	Less	-0.144	More	-1.627
MD	Less	-0.036	More	0.033	More	-0.838
MA	Less	0.025	More	0.046	More	-1.849
MI	More	-0.174	More	0.226	Less	-0.647
MN	More	-0.063	More	0.292	Less	-0.531
MS	More	-0.072	More	0.185	Less	-0.673
MO	Less	-0.059	More	0.118	More	-1.233
MT	More	-0.098	Less	-0.069	Less	-0.650
NE	More	-0.061	Less	-0.189	Less	-0.630
NV	More	-0.122	Less	0.031	Less	-0.608
NH	Less	0.031	Less	-0.476	More	-1.082
NJ	Less	0.001	Less	-0.154	More	-0.981
NM	Less	-0.043	Less	-0.080	More	-1.514
NY	Less	-0.004	More	0.119	Less	0.034
NC	Less	-0.059	Less	-0.375	More	-0.957
ND	More	-0.111	More	0.134	Less	-0.790
OH	More	-0.116	Less	-0.186	More	-0.969
OK	Less	0.071	More	0.260	More	-1.693
OR	More	-0.157	More	0.050	Less	-0.705
PA	Less	-0.061	More	0.205	More	-1.482
RI	Less	-0.001	More	0.384	More	-1.039
SC	More	-0.063	Less	-0.225	More	-1.332
SD	More	-0.115	Less	-0.247	Less	-0.711
TN	More	-0.085	Less	-0.147	More	-1.000
TX	Less	0.016	Less	-0.353	More	-1.347
UT	More	-0.101	More	0.094	Less	-0.739
VT	Less	-0.018	More	0.294	Less	-0.560

Table A.23: State-Level Heterogeneity

	State recession		State mean transfers		State transfer slope	
State (1)	More vs. less severe (2)	Change in log real earnings per capita, 1978-82 (3)	More vs. less generous (4)	Residual (5)	More vs. less progressive (6)	Slope (7)
VA	Less	-0.020	Less	-0.508	Less	-0.488
WA	More	-0.075	More	0.542	Less	-0.216
WV	More	-0.096	More	0.304	More	-1.131
WI	More	-0.094	More	0.234	More	-1.326
WY	More	-0.072	Less	-0.077	Less	-0.098

Notes: States with a more severe recession are those with an above-median decrease in log real earnings per capita from 1978-1982. States with less generous mean transfers are those with below-median transfers per capita in 1970, conditional on demographic and economic covariates. States with a less progressive transfer slope are those with an above-median slope coefficient from a regression of log transfers per capita on log median family income in 1970, conditional on demographic and economic covariates. See text for details. The mean (median) of column 3 is -0.059 (-0.061). The mean (median) of column 5 is 0 (0.031). The mean (median) of column 7 is -0.824 (-0.838).
Sources: BEA Regional Economic Accounts, Census County Business Patterns, Census County Data Book, U.S. Dept. of Education, National Center for Education Statistics (1978)

Table A.24: The Long-Run Effects of the 1980-1982 Recession on Additional Individual and Spousal Outcomes

	Dependent variable:				
	Migration from birth county (1)	Migration from birth state (2)	In labor force (3)	Positive hours worked (4)	Total hours worked (5)
Panel A: Interaction between 1978-1982 decrease in log real earnings per capita and age in 1979					
0-10	-0.121	-0.0186	0.220***	0.151***	112.2
	(0.0934)	(0.117)	(0.0567)	(0.0479)	(100.2)
11-19	-0.161*	-0.0547	0.299***	0.237***	426.0***
	(0.0916)	(0.0898)	(0.0784)	(0.0670)	(141.1)
20-28	-0.0363	0.0039	0.179***	0.146***	297.4**
	(0.0559)	(0.0554)	(0.0593)	(0.0561)	(125.8)
Panel B: Average value of dependent variable in years 2000-2013, by age in 1979, in levels					
0-10	-	0.353	0.844	0.856	1692
11-19	-	0.383	0.829	0.838	1692
20-28	-	0.399	0.790	0.802	1613

	Dependent variable:					
	Positive personal income (6)	Positive earned income (7)	Positive spousal income (8)	Personal income (9)	Earned income (10)	Spousal Income (11)
Panel A: Interaction between 1978-1982 decrease in log real earnings per capita and age in 1979						
0-10	0.0309	0.152***	-0.156*	9,324*	2,927	5,174
	(0.0287)	(0.0483)	(0.0888)	(5,237)	(4,771)	(4,107)
11-19	0.0929**	0.236***	-0.0652	-8,276	-11,799**	-462.3
	(0.0409)	(0.0673)	(0.0643)	(5,216)	(5,667)	(4,693)
20-28	0.0519*	0.148**	0.0760*	-5,051	-5,844	4,386
	(0.0289)	(0.0574)	(0.0399)	(3,838)	(3,644)	(3,435)
Panel B: Average value of dependent variable in years 2000-2013, by age in 1979, in levels						
0-10	0.910	0.855	0.319	42,728	41,004	21,592
11-19	0.910	0.838	0.388	51,325	48,484	27,176
20-28	0.916	0.801	0.505	54,198	48,988	31,581

Notes: See notes to Table 1.2. The sample in columns 1 and 2 contains 23.5 million individuals born from 1950-1979 in the continental U.S. with a unique birth county and non-imputed demographic and education variables. The sample in columns 3-11 contains 18.4 million individuals born from 1950-1979 in the continental U.S. with a unique birth county and non-imputed demographic, education, and labor market variables. Information on migration from birth county is not available from publicly available Census/ACS data, and I have not disclosed these statistics from the confidential Census/ACS data. For people born from 1950-1986 who were age 25-54 in 2000-2013, the average rate of migration from birth county is 0.687.

Sources: BEA Regional Economic Accounts, Census County Business Patterns, Confidential 2000-2013 Census/ACS data linked to the SSA NUMIDENT file, Publicly available 2000-2013 Census/ACS data from Ruggles et al. (2015)

Table A.25: The Long-Run Effects of the 1980-1982 Recession on Additional Family Outcomes

	Dependent variable:				
	Family income (1)	Income to poverty ratio $\times$ 100 (2)	Positive family income (3)	Married (4)	Family size (5)
Panel A: Interaction between 1978-1982 decrease in log real earnings per capita and age in 1979					
0-10	-5,573	-17.18	-0.0068	0.114**	1.874***
	(11,576)	(64.97)	(0.0117)	(0.0450)	(0.544)
11-19	-25,028*	2.577	-0.0036	-0.107*	0.384
	(12,818)	(56.16)	(0.0117)	(0.0647)	(0.277)
20-28	-10,010	32.89	0.0013	-0.0109	-0.206*
	(7,604)	(40.30)	(0.0120)	(0.0440)	(0.107)
Panel B: Average value of dependent variable in years 2000-2013, by age in 1979					
0-10	80,971	412.8	0.977	0.585	3.19
11-19	94,026	468.2	0.977	0.661	3.19
20-28	98,311	543.2	0.979	0.679	2.65

Notes: See notes to Table 1.2. The sample contains 18.4 million individuals born from 1950-1979 in the continental U.S. with a unique birth county and non-imputed education and labor market variables.
Sources: BEA Regional Economic Accounts, Census County Business Patterns, Confidential 2000-2013 Census/ACS data linked to the SSA NUMIDENT file, Publicly available 2000-2013 Census/ACS data from Ruggles et al. (2015)

Table A.26: Summary Statistics, Across Birth Counties

	Percentile						
	5	25	50	75	95	Mean	SD
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Education outcomes							
HS/GED attainment	0.756	0.843	0.903	0.939	0.967	0.884	0.073
Any college attendance	0.325	0.456	0.549	0.634	0.715	0.538	0.121
Any college degree attainment	0.153	0.241	0.310	0.374	0.461	0.306	0.095
Four-year college degree attainment	0.098	0.167	0.216	0.268	0.357	0.217	0.077
Two-year degree attainment	0.043	0.067	0.087	0.109	0.146	0.089	0.032
Years of education	12.06	12.70	13.14	13.49	13.92	13.07	0.59
Labor market outcomes							
Log personal income	10.28	10.40	10.47	10.53	10.67	10.46	0.127
Log earned income	10.22	10.34	10.41	10.48	10.61	10.41	0.128
Log hourly wage	2.754	2.846	2.909	2.970	3.113	2.912	0.110
Log family income	10.83	10.97	11.05	11.13	11.25	11.04	0.137
In poverty	0.051	0.081	0.107	0.142	0.211	0.117	0.056
Migration from birth county	0.518	0.625	0.701	0.777	0.896	0.700	0.112
Migration from birth state	0.136	0.226	0.303	0.402	0.581	0.321	0.134
In labor force	0.711	0.784	0.824	0.856	0.905	0.816	0.062
Positive hours worked	0.728	0.802	0.843	0.876	0.924	0.835	0.063
Total hours worked	1419	1597	1692	1783	1965	1687	165
Positive personal income	0.870	0.902	0.918	0.935	0.959	0.916	0.033
Positive earned income	0.726	0.800	0.841	0.873	0.920	0.832	0.062
Positive spousal income	0.373	0.433	0.467	0.502	0.561	0.465	0.063
Personal income	30,921	36,883	41,009	44,703	51,505	40,839	6,516
Earned income	27,940	34,300	38,624	42,277	48,717	38,277	6,501
Spousal income	14,414	18,700	20,726	22,694	25,868	20,502	3,835
Family income	56,080	66,807	74,172	80,555	91,572	73,673	11,067
Income to poverty ratio $\times$ 100	301.9	356.5	395.5	429.2	491.0	393.4	58.8
Positive family income	0.956	0.973	0.981	0.986	0.994	0.978	0.015
Married	0.583	0.671	0.707	0.739	0.787	0.699	0.064
Family size	2.59	2.80	2.88	2.95	3.18	2.88	0.19

Notes: Table reports summary statistics for outcomes at the birth county level. I collapse variables across years 2000-2013 and across cohorts 1950-1979. To ensure that no confidential information is disclosed, I estimate the 5th percentile as the average among counties in percentiles 4-6; other percentiles are calculated similarly.
Source: Confidential 2000-2013 Census/ACS data linked to the SSA NUMIDENT file

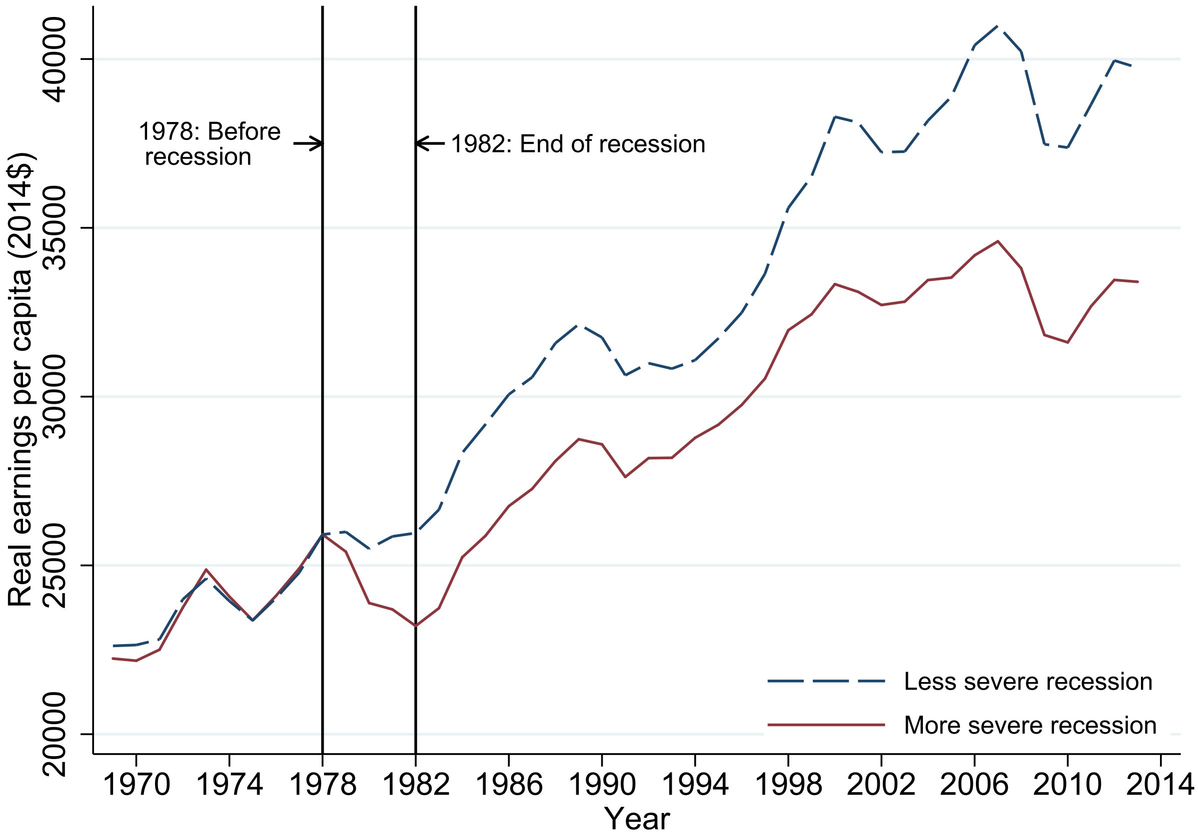
Table A.27: Cross-Sectional Relationship between Average Long-Run Outcome and Earnings per Capita in Birth County in 1978

	Log earnings per capita, 1978			
Dependent variable	Coefficient	Standard Error	R^2	Observations
HS/GED attainment	0.152***	(0.0154)	0.266	3,074
Any college attendance	0.273***	(0.0237)	0.315	3,074
Any college degree attainment	0.217***	(0.0170)	0.322	3,074
Four-year college degree attainment	0.177***	(0.0127)	0.325	3,074
Two-year college degree attainment	0.0396***	(0.00690)	0.093	3,074
Years of education	1.374***	(0.111)	0.335	3,074
Log personal income	0.290***	(0.0174)	0.323	3,071
Log earned income	0.287***	(0.0177)	0.310	3,071
Log wage	0.254***	(0.0212)	0.326	3,071
Log family income	0.334***	(0.0206)	0.364	3,071
In poverty	-0.121***	(0.0111)	0.287	3,072
Migration from birth county	0.103***	(0.0167)	0.052	3,074
Migration from birth state	0.141***	(0.0235)	0.068	3,074
In labor force	0.124***	(0.0143)	0.245	3,072
Positive hours worked	0.132***	(0.0151)	0.274	3,072
Total hours worked	319.5***	(38.85)	0.231	3,072
Positive personal income	0.0430***	(0.00572)	0.103	3,072
Positive earned income	0.131***	(0.0150)	0.275	3,072
Positive spousal income	0.0267	(0.0184)	0.011	3,072
Personal income	17,375***	(988.1)	0.438	3,072
Earned income	17,562***	(1,003)	0.450	3,072
Spousal income	7,472***	(569.9)	0.234	3,072
Family income	30,187***	(1,748)	0.458	3,072
Income to poverty ratio $\times$ 100	158.4***	(8.379)	0.446	3,072
Positive family income	0.0217***	(0.00279)	0.138	3,072
Married	0.0387*	(0.0197)	0.023	3,072
Small family size	0.0858**	(0.0351)	0.012	3,072

Notes: Table reports bivariate regressions of average long-run outcome for a birth county, averaged across survey years 2000-2013 and birth cohorts 1950-1979, on log real earnings per capita in that county in 1978. Standard errors clustered by state.

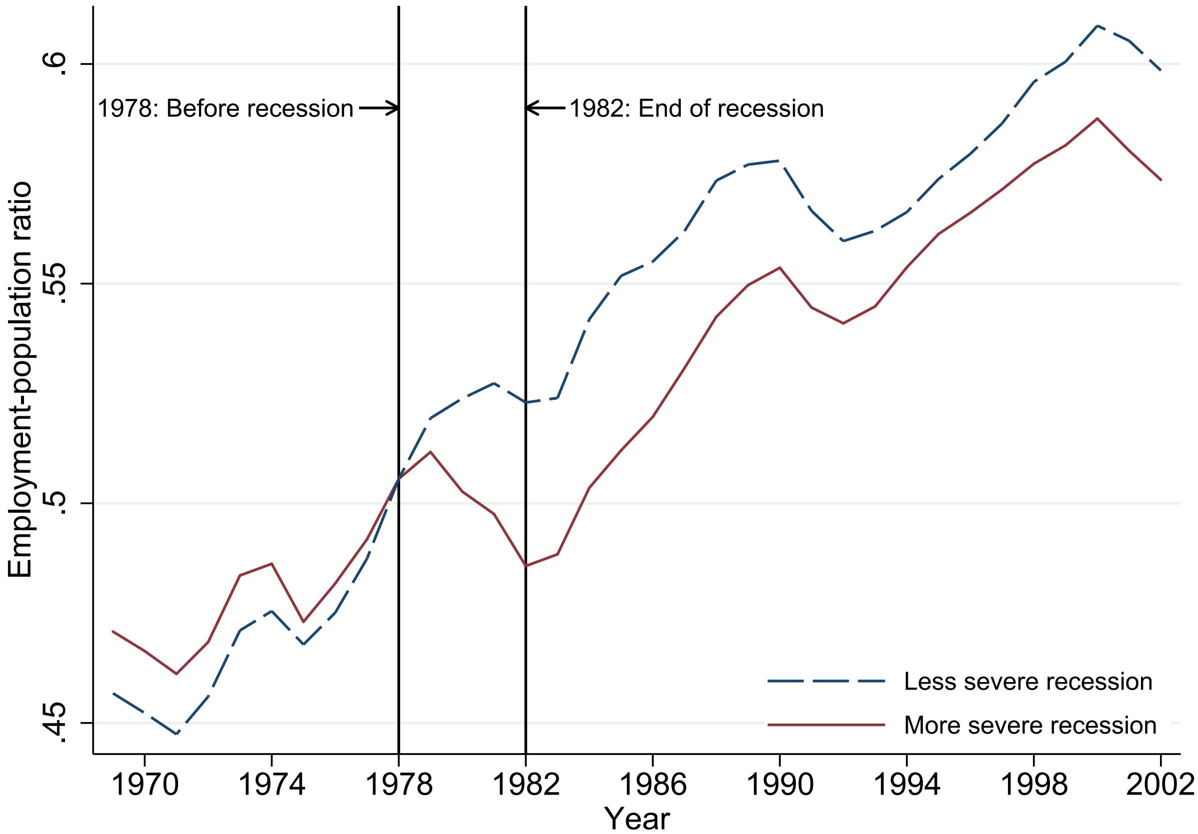
Sources: BEA Regional Economic Accounts, Census County Business Patterns, Confidential 2000-2013 Census/ACS data linked to the SSA NUMIDENT file

Figure A.1: Normalized Mean Real Earnings per Capita, by County-Level Severity of the 1980-1982 Recession, 1969-2013



Notes: Figure extends the data in Figure 1.1 from 2002-2013. See notes to Figure 1.1.
Source: BEA Regional Economic Accounts

Figure A.2: Normalized Mean Employment-Population Ratio, by County-Level Severity of the 1980-1982 Recession

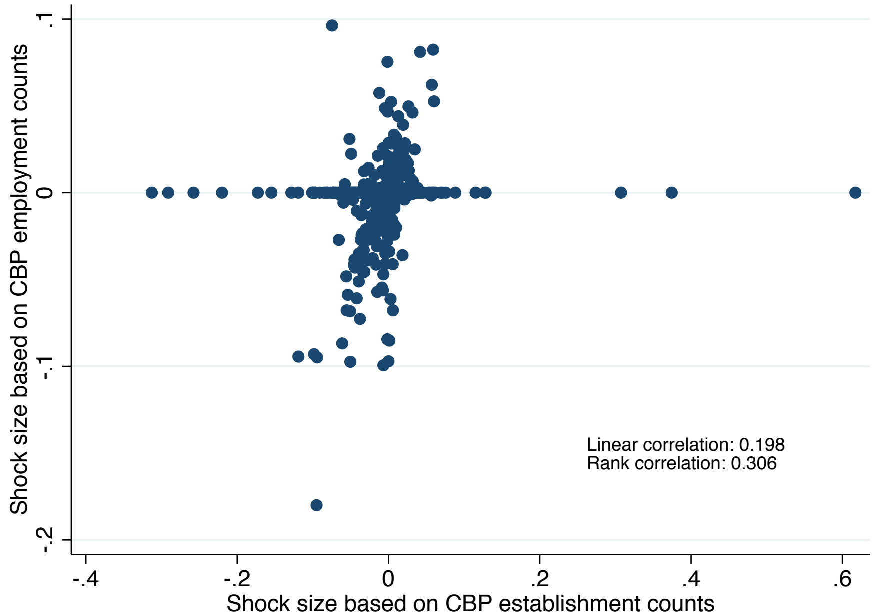


Notes: Figure displays the population-weighted mean employment-population ratio, among counties with a below and above median 1978-1982 decrease in log real earnings per capita. I calculate the median using 1978 population weights. I adjust the less severe recession line to equal the more severe recession line in 1978, which amounts to an upward shift of 0.024. Sample contains 3,076 counties in the continental U.S.
Source: BEA Regional Economic Accounts

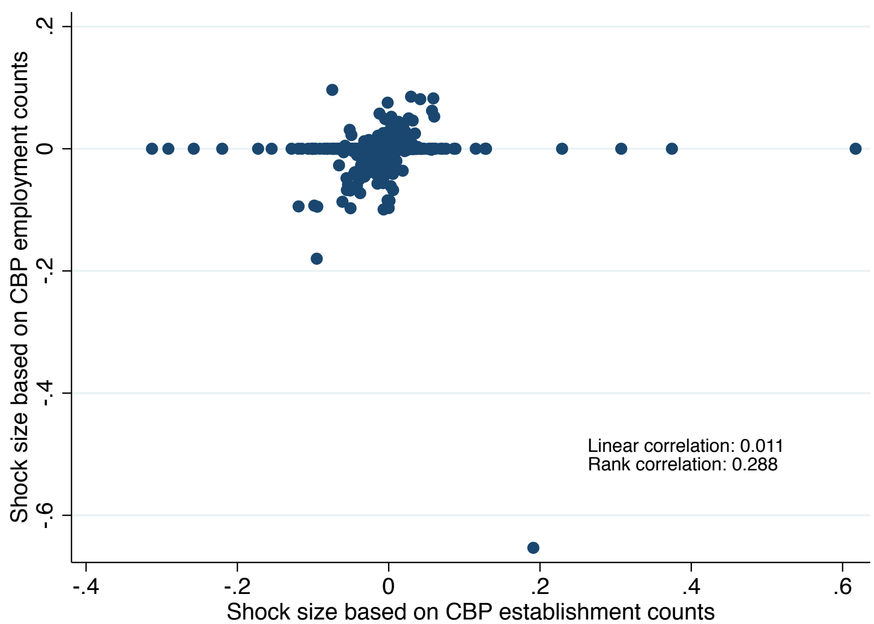
Figure A.3: Distribution of County-Level Log Real Earnings per Capita Change, 1978-1982

Notes: Sample limited to 3,076 counties in the continental U.S.
Source: BEA Regional Economic Accounts

Figure A.4: The Role of County Business Patterns Employment Suppression in Constructing the Shock Size Variable used by Feyrer, Sacerdote and Stern (2007)



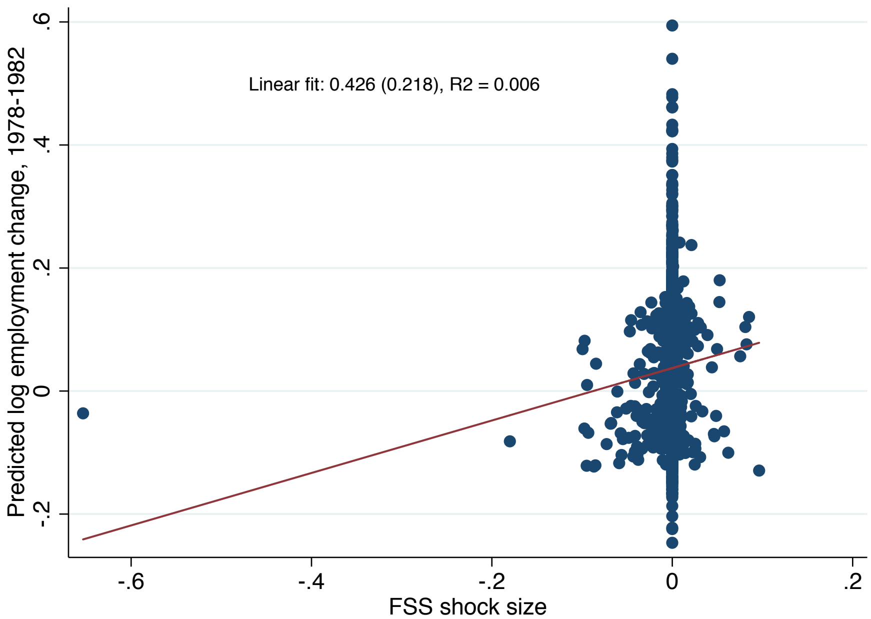
(a) Counties with at least 10,000 residents in 1977 (FSS sample)



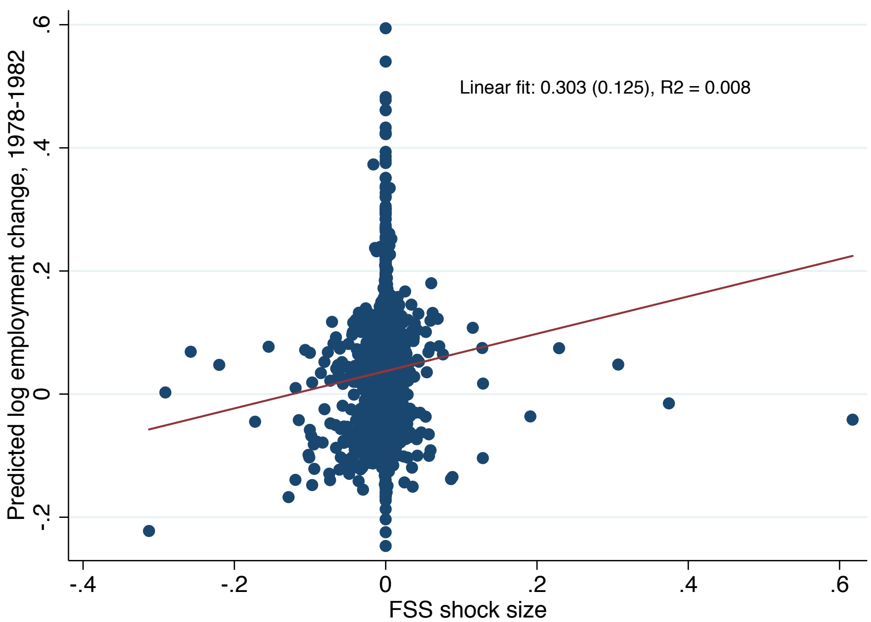
(b) All counties

Notes: Shock size is the 1977-1982 employment change in the auto and steel industries divided by 1977 total employment. FSS construct the shock size based on CBP employment counts, which are frequently suppressed. An alternative approach is to use CBP establishment counts, which are never suppressed. See text for details.
Sources: BLS Local Area Statistics and Census County Business Patterns

Figure A.5: Comparison of Predicted Log Employment Change to Shock Size Variable used by Feyrer, Sacerdote and Stern (2007)



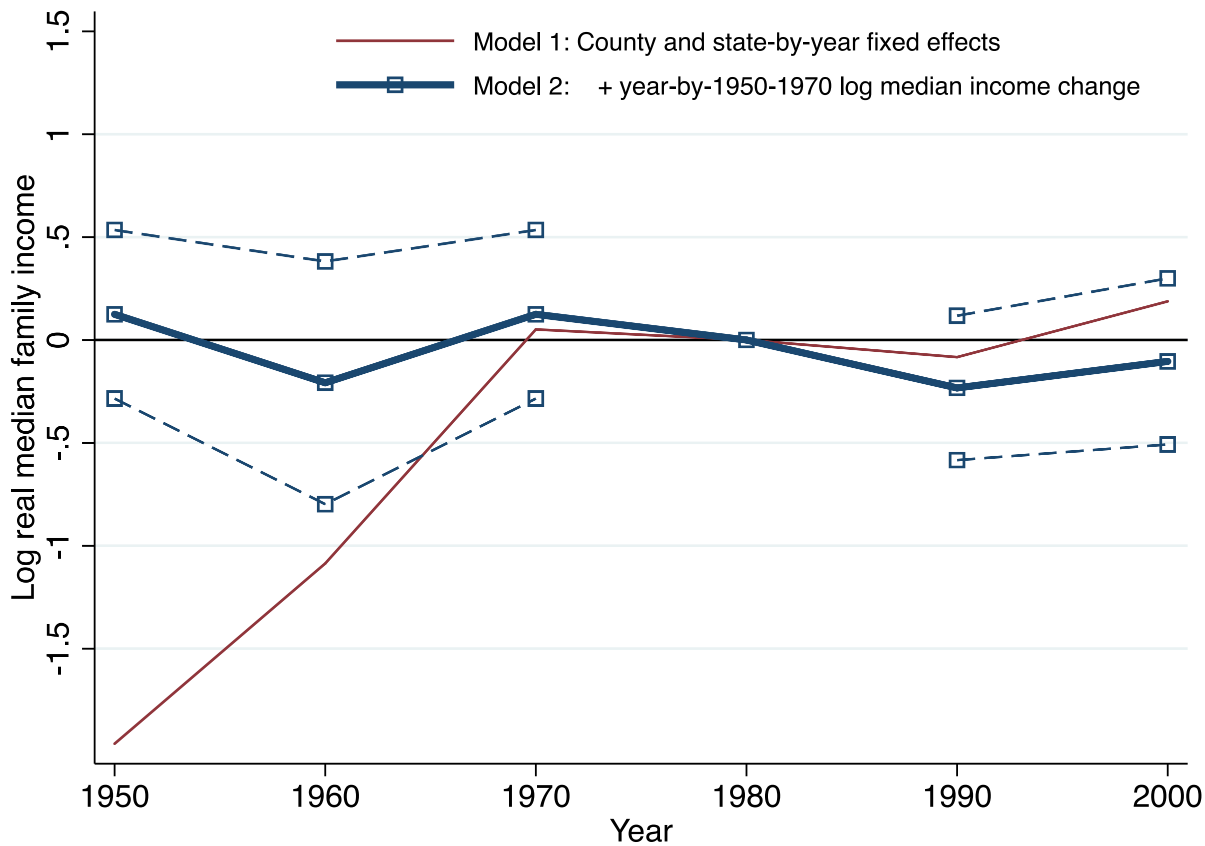
(a) Using CBP Employment Counts to Construct Shock Size



(b) Using CBP Establishment Counts to Construct Shock Size

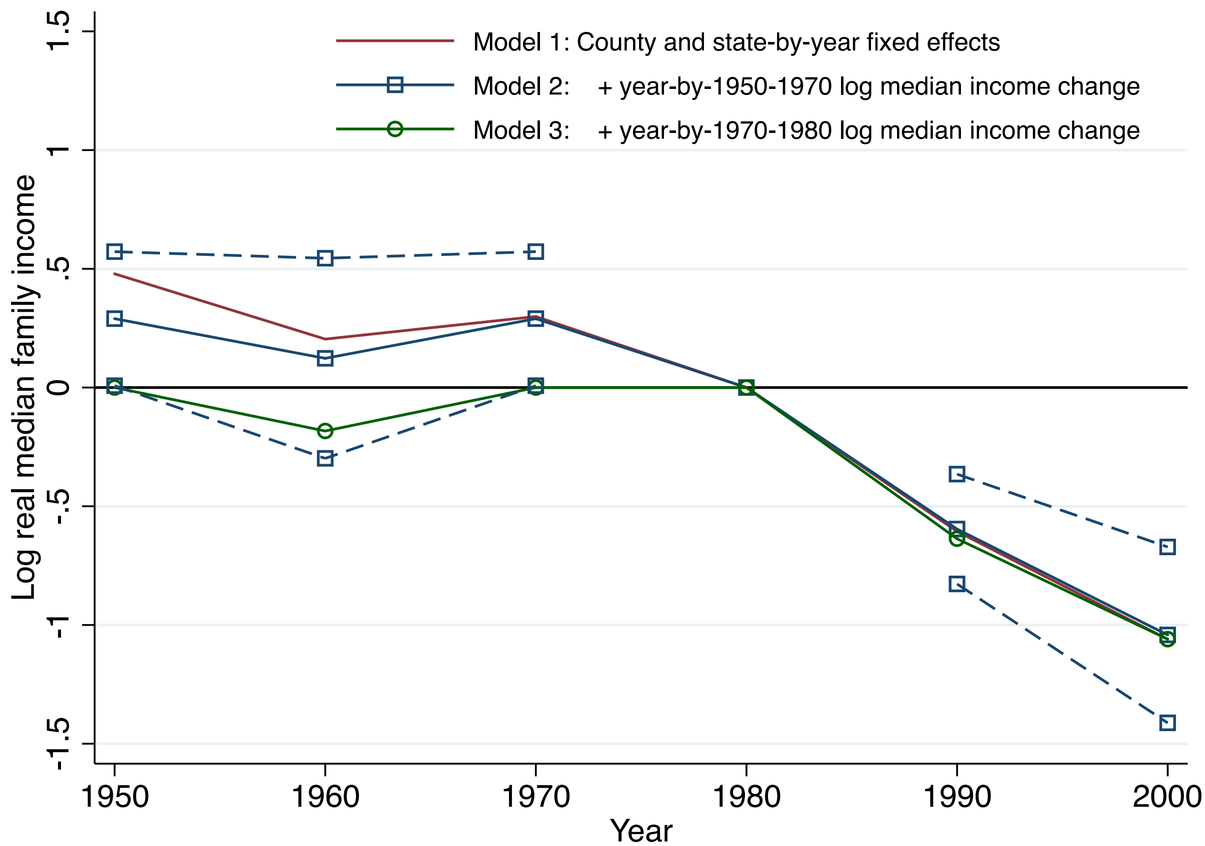
Notes: Predicted log employment change is based on a county's 1976 industrial structure and aggregate industry-level employment changes, as defined in equation (1.1). Shock size is the 1977-1982 employment change in the auto and steel industries divided by 1977 total employment. Panel A constructs the shock size variable using CBP employment counts, as in FSS. Panel B uses CBP establishment counts. Standard errors are clustered by state.
Sources: BLS Local Area Statistics and Census County Business Patterns data

Figure A.6: Log Real Median Family Income Before and After the 1980-1982 Recession, 2SLS Estimates, Including Counties with High Mining Employment Share



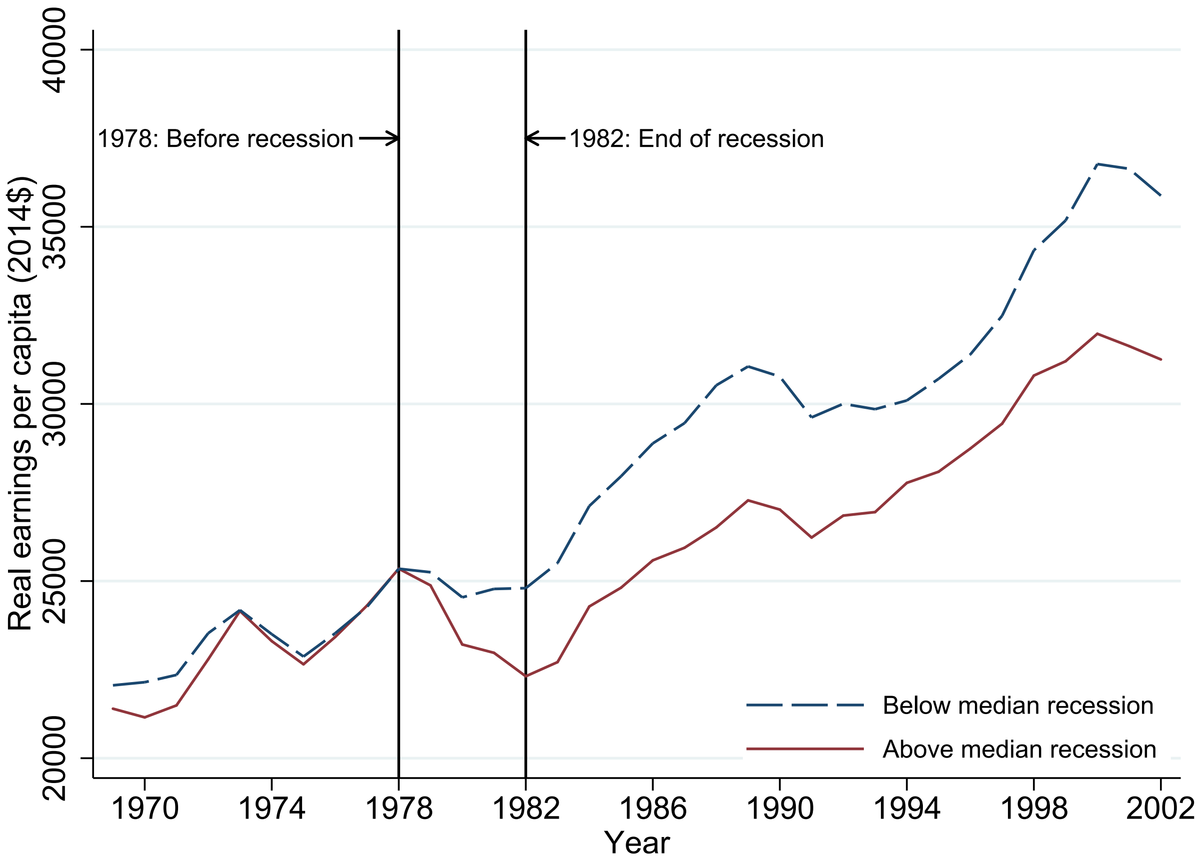
Notes: See notes to Figure 1.4. Sample contains 3,076 counties in the continental U.S.
Sources: BEA Regional Economic Accounts, Census County Business Patterns, Census County Data Books, Minnesota Population Center (2011)

Figure A.7: Log Real Median Family Income Before and After the 1980-1982 Recession, 2SLS Estimates, Measuring Recession Severity at Commuting Zone Level



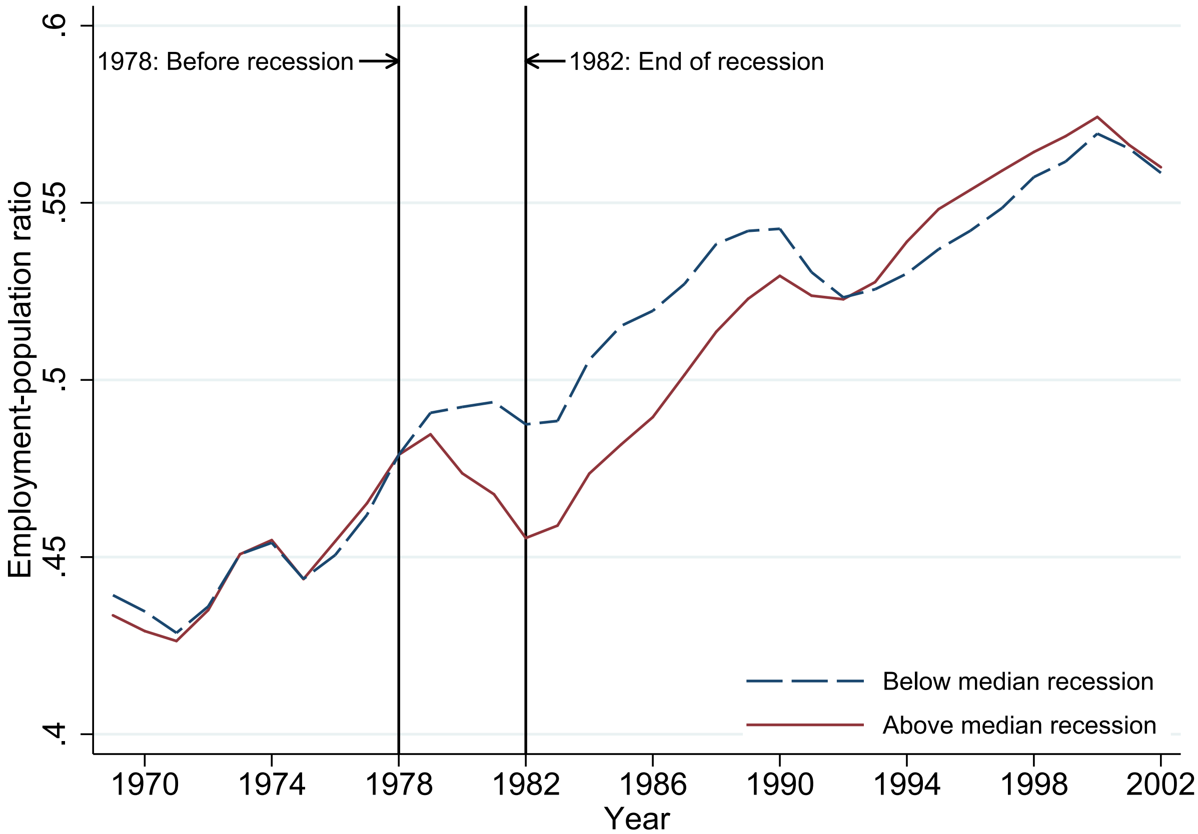
Notes: Figure plots the estimated coefficients on interactions between year and the 1978-1982 decrease in log real earnings per capita, where the coefficient for 1980 is normalized to equal zero. The dependent variable is log real median family income for 1950-1990 and log real median household income for 2000. Regressions are estimated by 2SLS, using the predicted log employment change from 1978-1982 as an instrumental variable. The change in log earnings per capita and the predicted employment change are measured at the commuting zone level. The dashed lines are pointwise 95 percent confidence intervals based on standard errors clustered by state. Sample is limited to the 2,550 counties with less than 5 percent of 1976 employment in the mining sector.
Sources: BEA Regional Economic Accounts, Census County Business Patterns, Census County Data Books, Minnesota Population Center (2011)

Figure A.8: Normalized Mean Real Earnings per Capita, by Commuting Zone-Level Severity of the 1980-1982 Recession



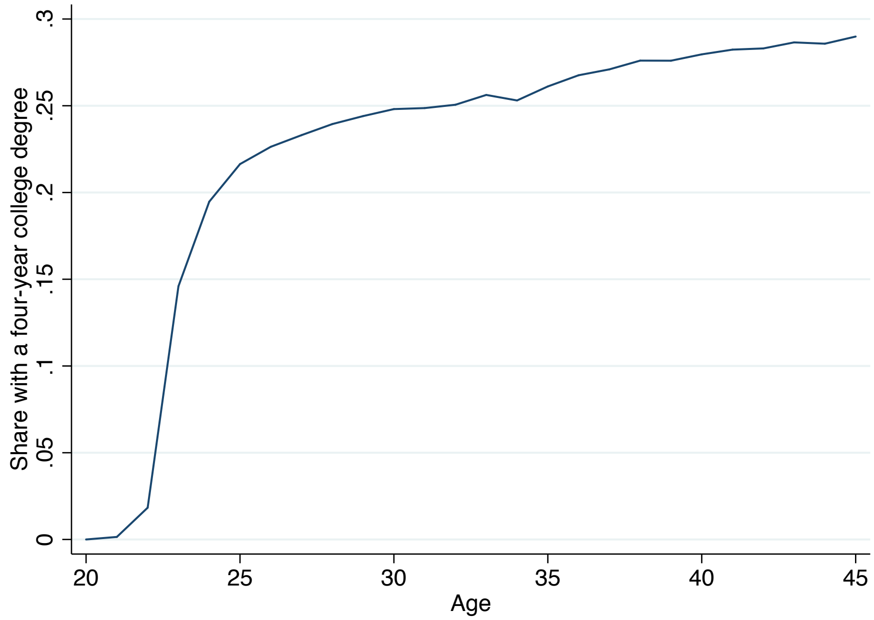
Note: Figure displays population-weighted mean real earnings per capita, among commuting zones with a below and above median 1978-1982 decrease in log real earnings per capita. I calculate the median using 1978 population weights. I adjust the less severe recession line to equal the more severe recession line in 1978, which amounts to a downwards shift of $2,361. Sample contains 722 commuting zones in the continental U.S.
Source: BEA Regional Economic Accounts

Figure A.9: Normalized Mean Employment-Population Ratio, by Commuting-Zone Level Severity of the 1980-1982 Recession

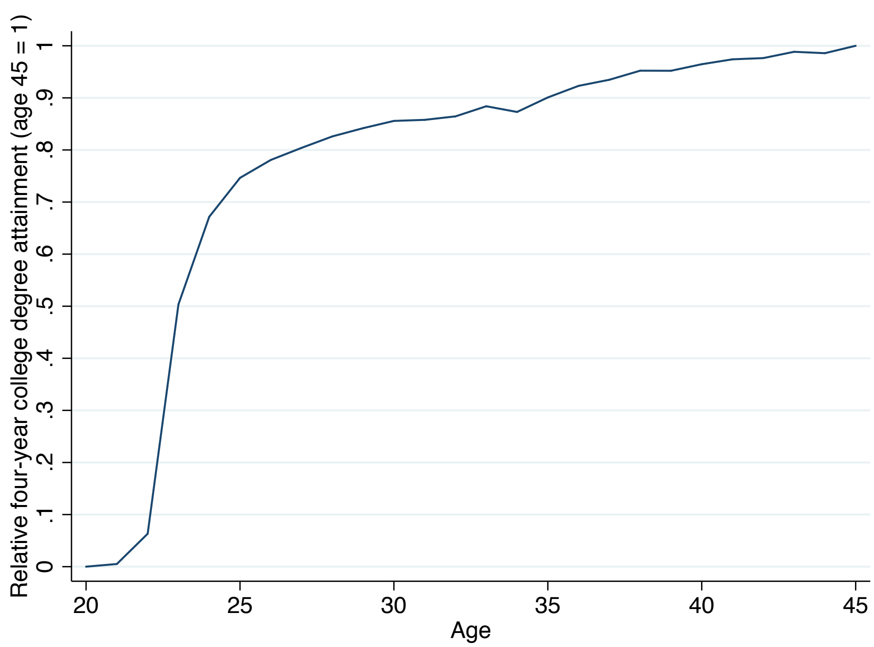


Note: Figure displays the population-weighted mean employment-population ratio, among commuting zones with a below and above median 1978-1982 decrease in log real earnings per capita. I calculate the median using 1978 population weights. I adjust the less severe recession line to equal the more severe recession line in 1978, which amounts to a downwards shift of 0.021. Sample contains 722 commuting zones in the continental U.S.
Source: BEA Regional Economic Accounts

Figure A.10: Four-Year College Degree Attainment, by Age



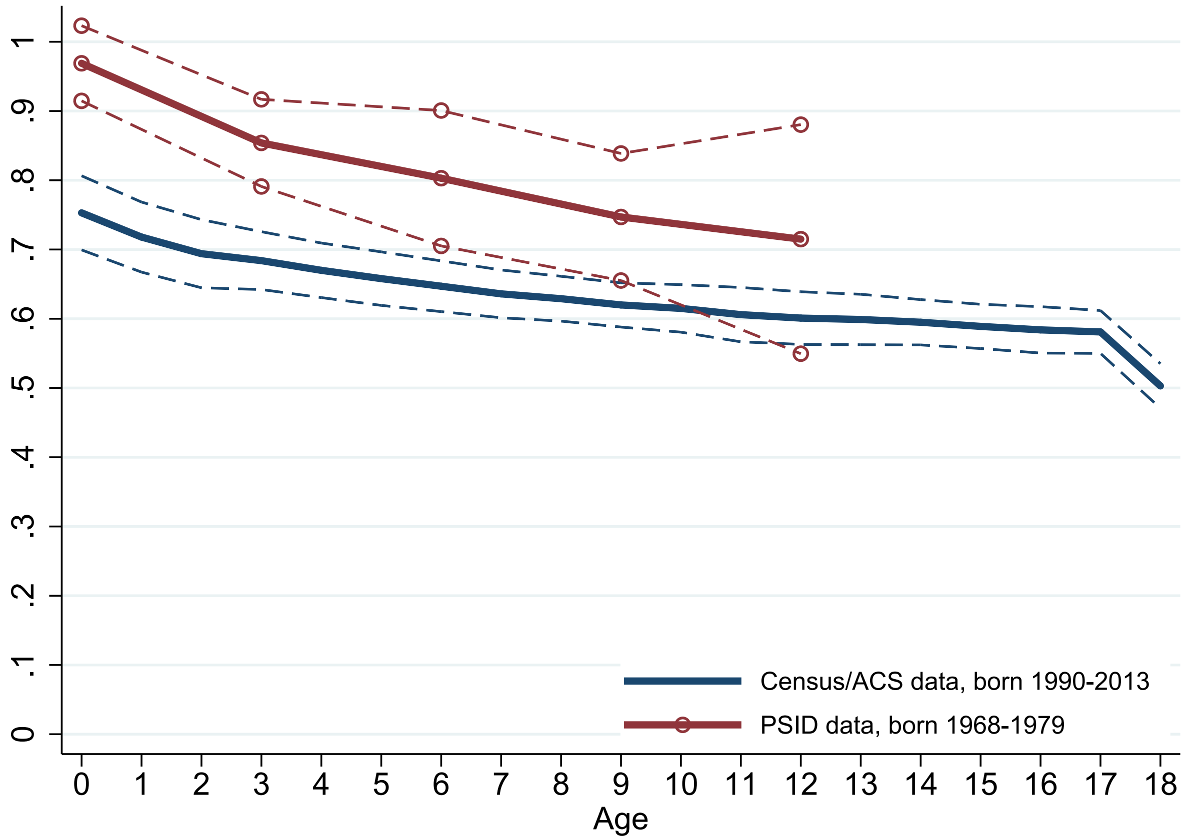
(a) Share with a Four-Year College Degree



(b) Share with a Four-Year College Degree, Relative to Age 45 Attainment

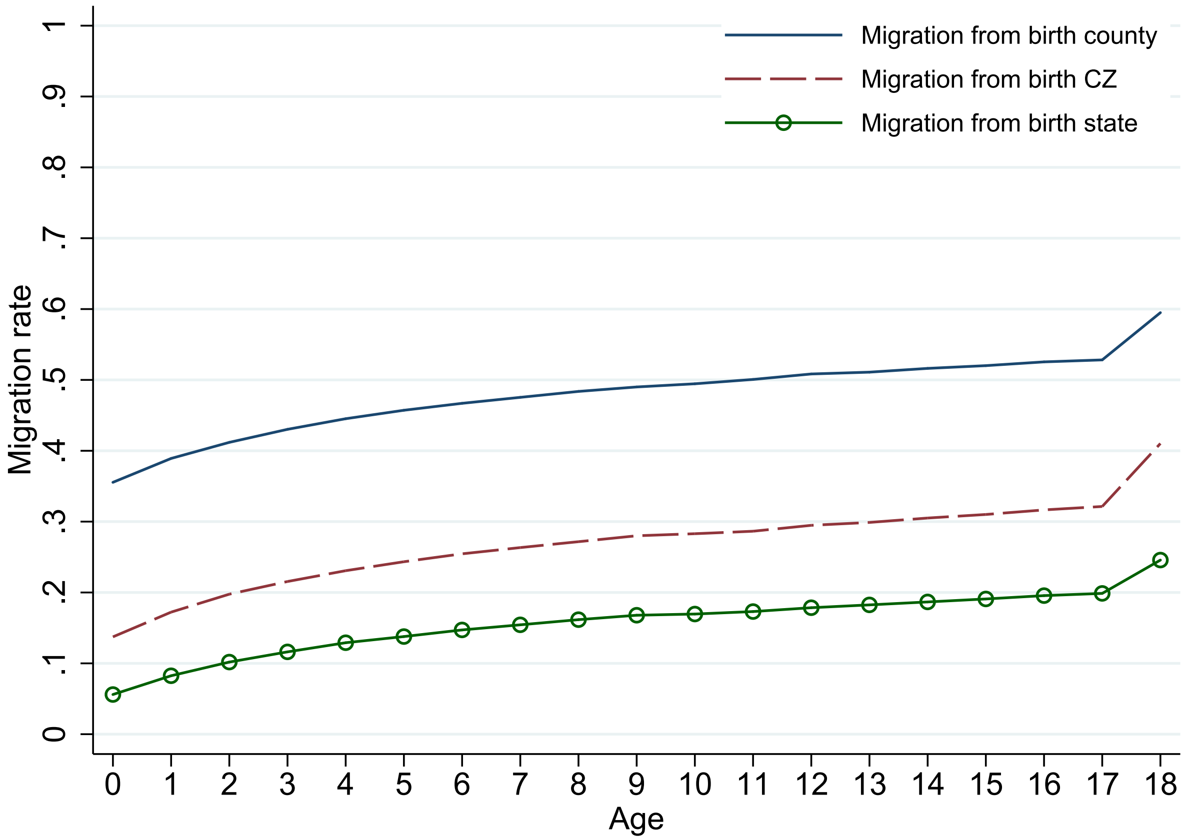
Notes: Panel A displays the share of individuals with a four-year college degree, for a constant sample of individuals born in the U.S. from 1957-1964. Panel B displays the share of attainment divided by attainment at age 45. I use custom weights from the NLS to account for the fact that these tabulations use multiple years of data.
Source: National Longitudinal Survey of Youth 1979 (1979-2010)

Figure A.11: Relationship between Severity of 1980-1982 Recession in County of Residence and County of Birth



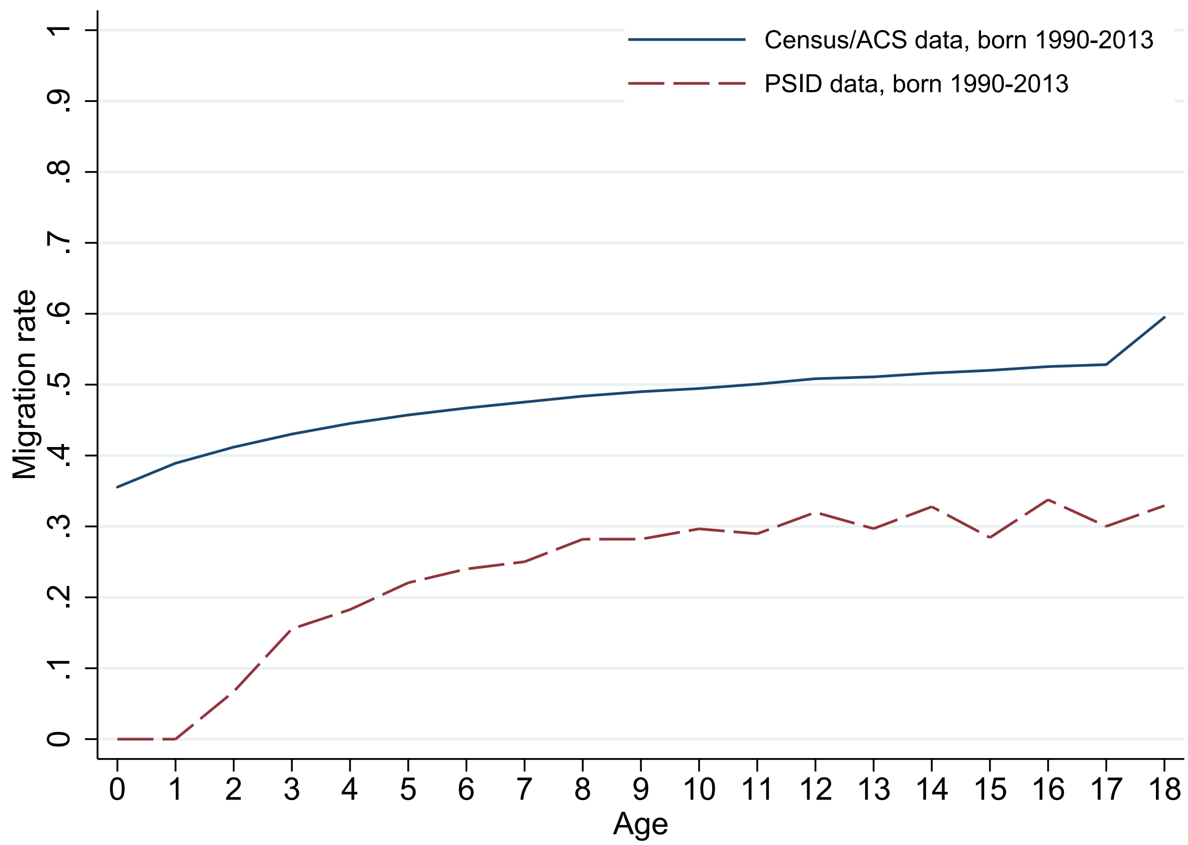
Notes: Figure plots OLS estimates of the interaction between the 1978-1982 decrease in log real earnings per capita in individuals' county of birth and indicators for age. The dependent variable is the 1978-1982 decrease in log real earnings per capita in individuals' county of residence. I estimate this relationship using confidential Census/ACS data for individuals born from 1990-2013 and confidential PSID data for individuals born from 1968-1979. All regressions include fixed effects for birth year-by-birth state and birth-year interacted with the 1950-1970 change in log median family income in individuals' birth county. The Census/ACS regression also includes fixed effects for race, sex, and survey year. The dashed lines are pointwise 95 percent confidence intervals based on standard errors clustered by state. The Census/ACS sample contains 11.7 million individuals born in the continental U.S. from 1990-2013 with a unique birth county and non-imputed variables. The PSID sample contains 3,684 individuals born in the continental U.S. from 1968-1979.
Sources: BEA Regional Economic Accounts, Confidential 2000-2013 Census/ACS data linked to the SSA NUMIDENT file, Confidental PSID data

Figure A.12: Out-Migration Rates by Age



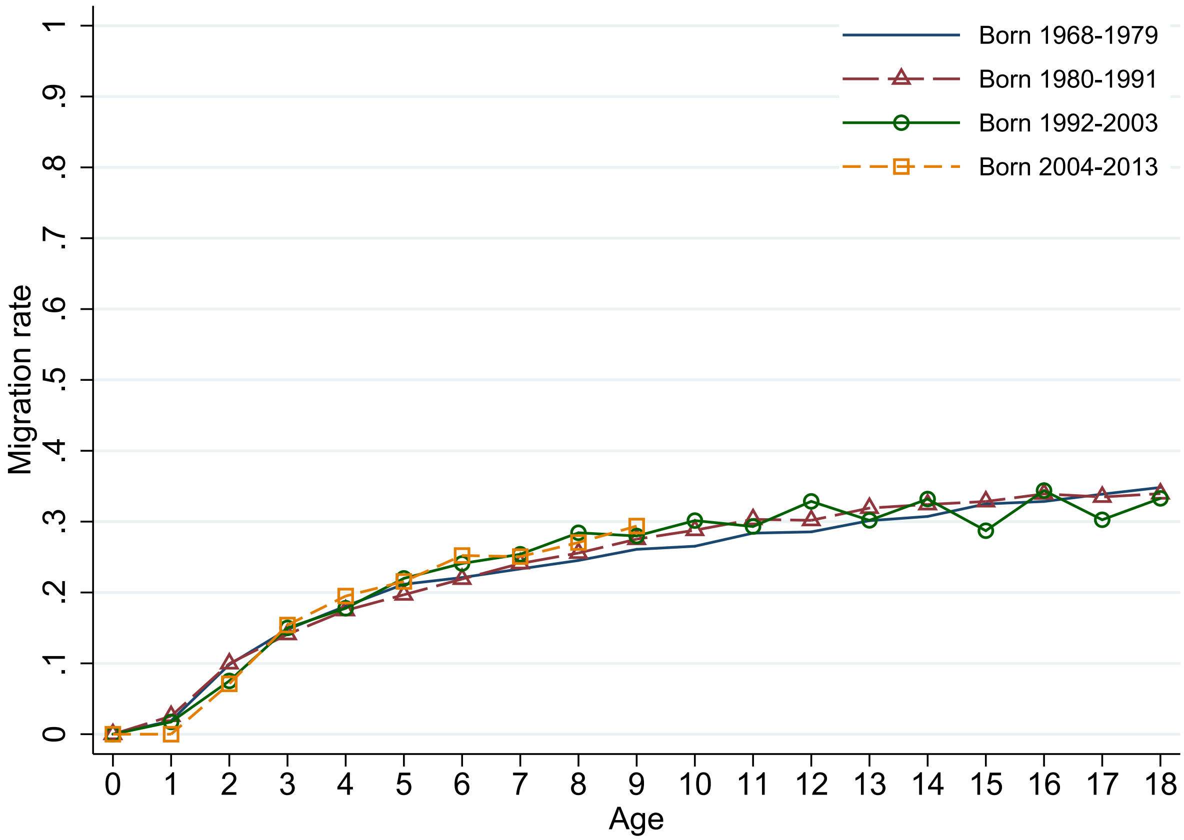
Notes: Figure displays the share of individuals living outside of their birth county, commuting zone, and state. The Census/ACS data provide information on county of birth, but not county of residence at time of birth. The sample contains 11.7 million individuals born in the continental U.S. from 1990-2013 with a unique birth county and non-imputed variables. For reference, birth certificate data for individuals born in 1990, 1995, and 2000 indicate that 18.6 percent of individuals are born outside their county of residence and 2.3 percent of individuals are born outside their state of residence.
Source: Confidential 2000-2013 Census/ACS data linked to the SSA NUMIDENT file

Figure A.13: Comparison of Birth County Out-Migration Rates by Data Source



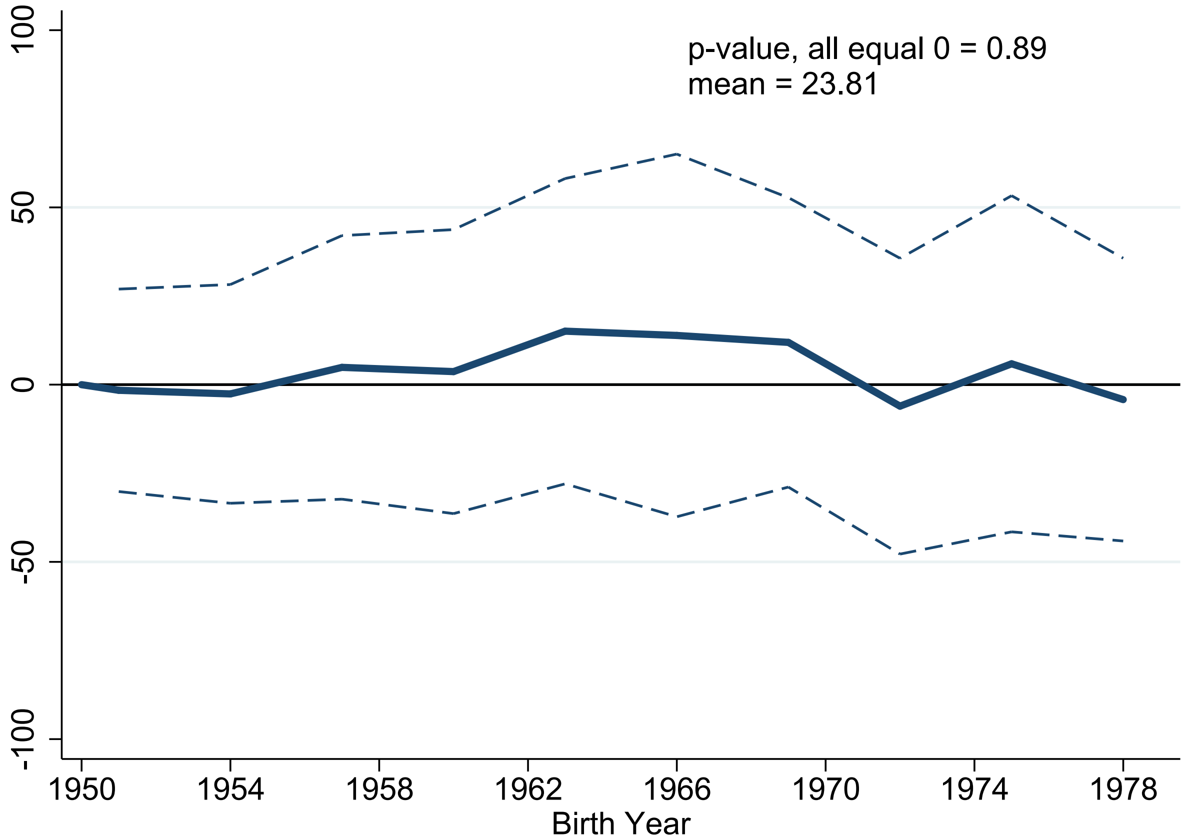
Notes: The Census/ACS data provide information on county of birth, but not county of residence at time of birth. The PSID data provide information on county of residence (where the interview took place) during infancy. The Census/ACS sample contains 11.7 million individuals born in the continental U.S. from 1990-2013 with a unique birth county and non-imputed variables. The PSID sample contains 32,295 person-year observations for individuals born in the continental U.S. from 1990-2013.
Sources: Confidential 2000-2013 Census/ACS data linked to the SSA NUMIDENT file, Confidental PSID data

Figure A.14: Comparison of Birth County Out-Migration Rates by Cohort



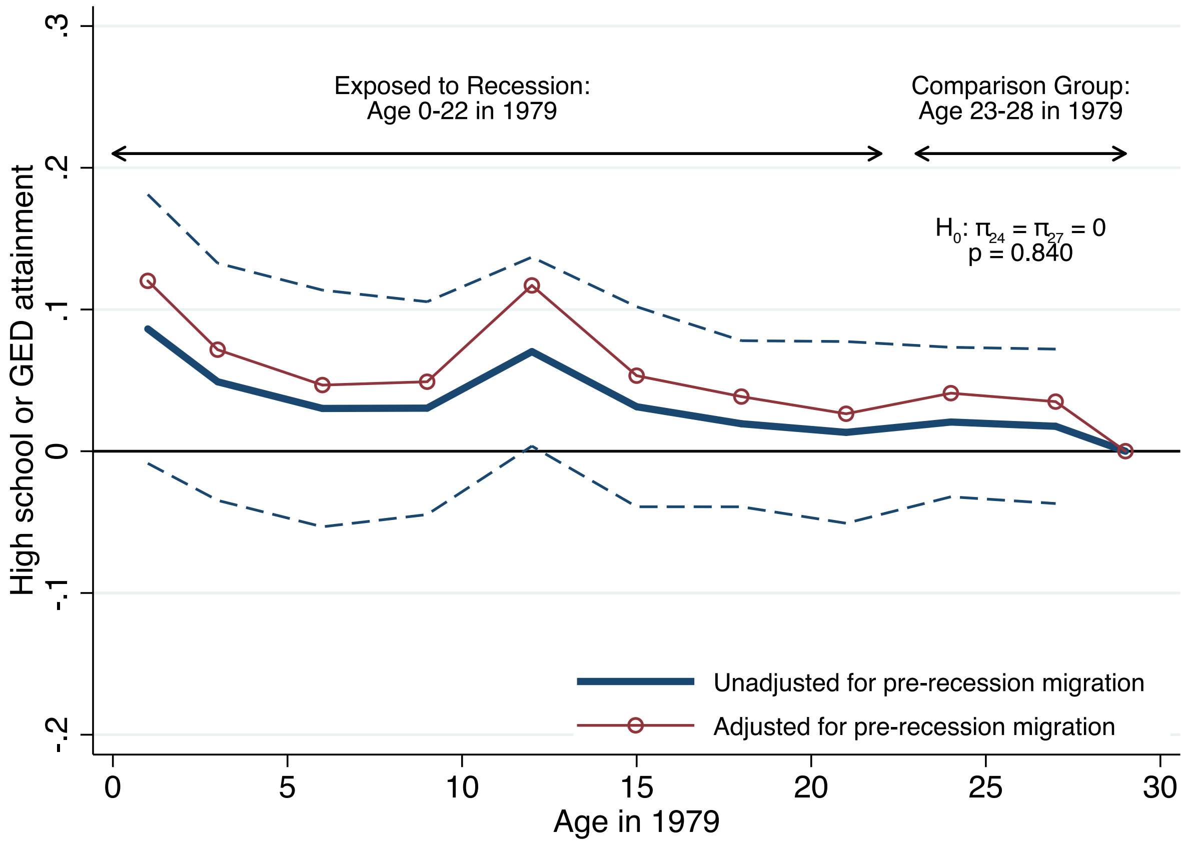
Notes: Figure displays the share of individuals living outside of their birth county for different birth cohorts. The PSID data provide information on county of residence (where the interview took place).
Source: Confidental PSID data

Figure A.15: Infant Mortality Did Not Evolve Differentially Before the 1980-1982 Recession



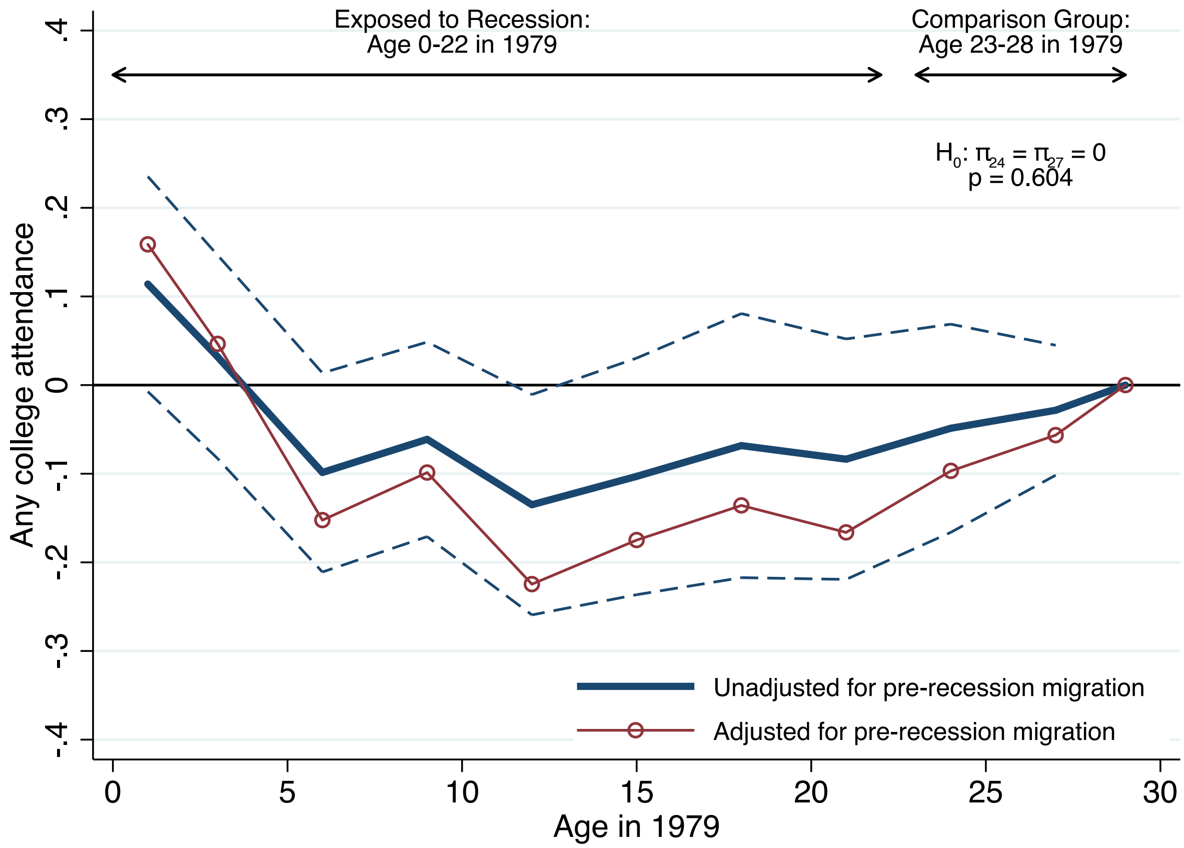
Notes: Figure plots the estimated coefficients on interactions between birth year and the 1978-1982 decrease in log real earnings per capita, where the coefficient for 1950 is normalized to equal zero. The dependent variable is the infant mortality rate (deaths pers 1,000 births). The regression is estimated by 2SLS, using the predicted log employment change from 1978-1982 as an IV. The regression includes fixed effects for birth county and birth year-by-birth state, plus interactions between birth year and the 1950-1970 change in log median family income. The dashed lines are pointwise 95 percent confidence intervals based on standard errors clustered at the birth county level. Sample is limited to the 2,550 counties with less than 5 percent of 1976 employment in the mining sector.
Sources: Bailey et al. (2016), BEA Regional Economic Accounts, Census County Business Patterns, Census County Data Books, Minnesota Population Center (2011)

Figure A.16: The Long-Run Effects of the 1980-1982 Recession on High School or GED Attainment



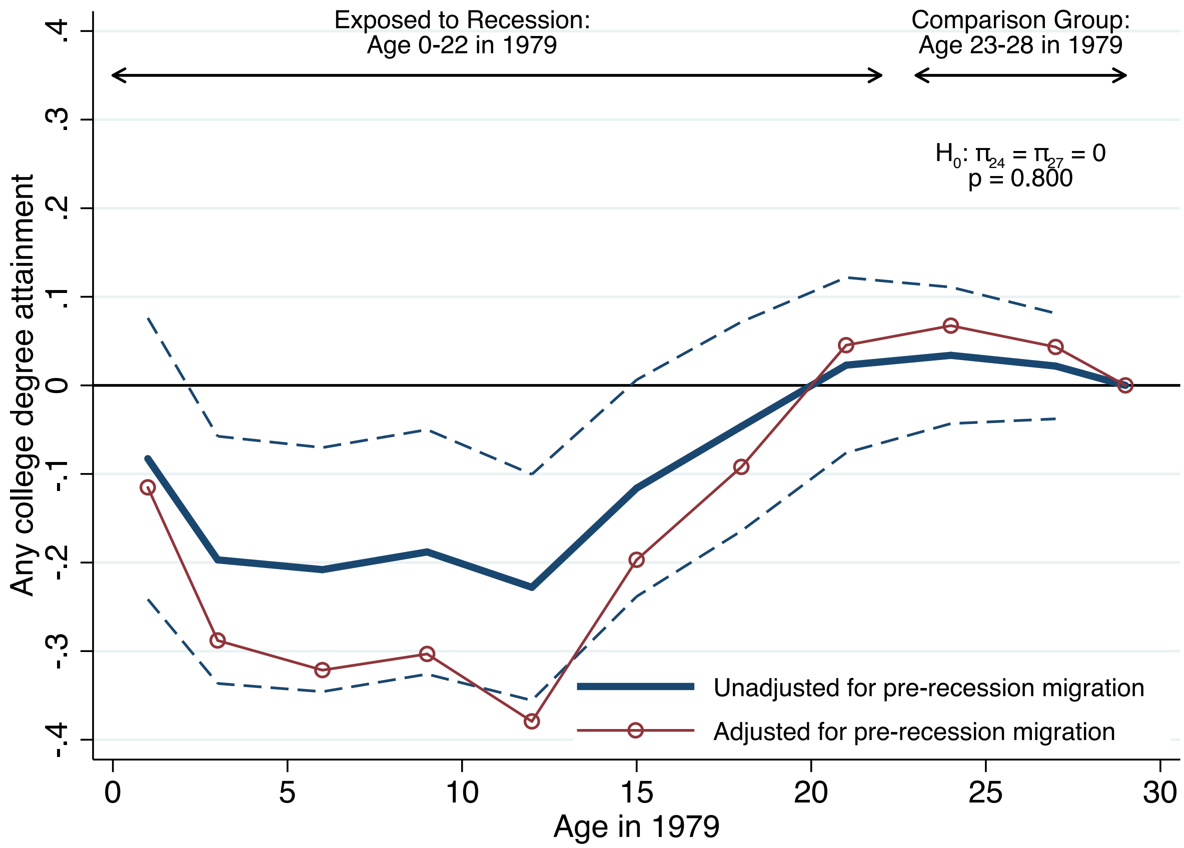
Notes: See notes to Figure 1.6. The dependent variable is an indicator for high school or GED attainment.
Sources: BEA Regional Economic Accounts, Census County Business Patterns, Confidential 2000-2013 Census/ACS data linked to the SSA NUMIDENT file

Figure A.17: The Long-Run Effects of the 1980-1982 Recession on Any College Attendance



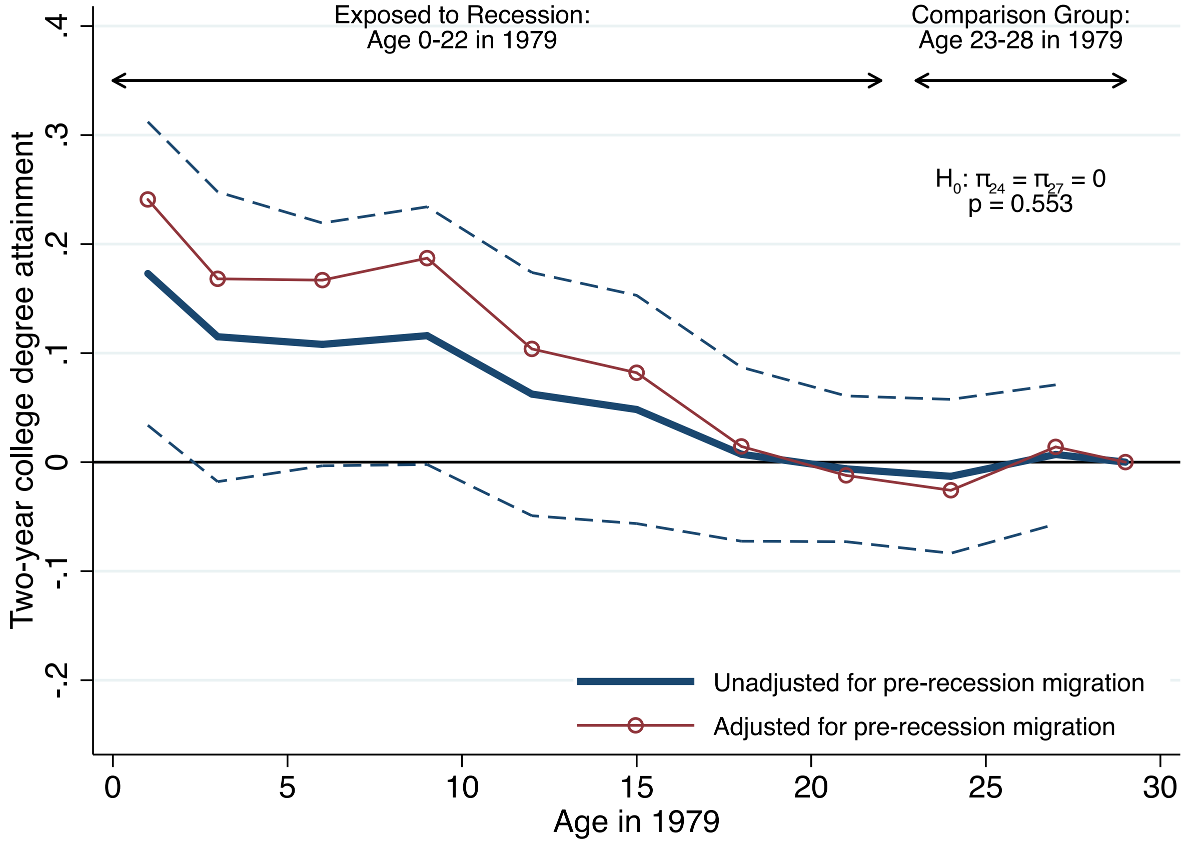
Notes: See notes to Figure 1.6. The dependent variable is an indicator for any college attendance.
Sources: BEA Regional Economic Accounts, Census County Business Patterns, Confidential 2000-2013 Census/ACS data linked to the SSA NUMIDENT file

Figure A.18: The Long-Run Effects of the 1980-1982 Recession on Any College Degree Attainment



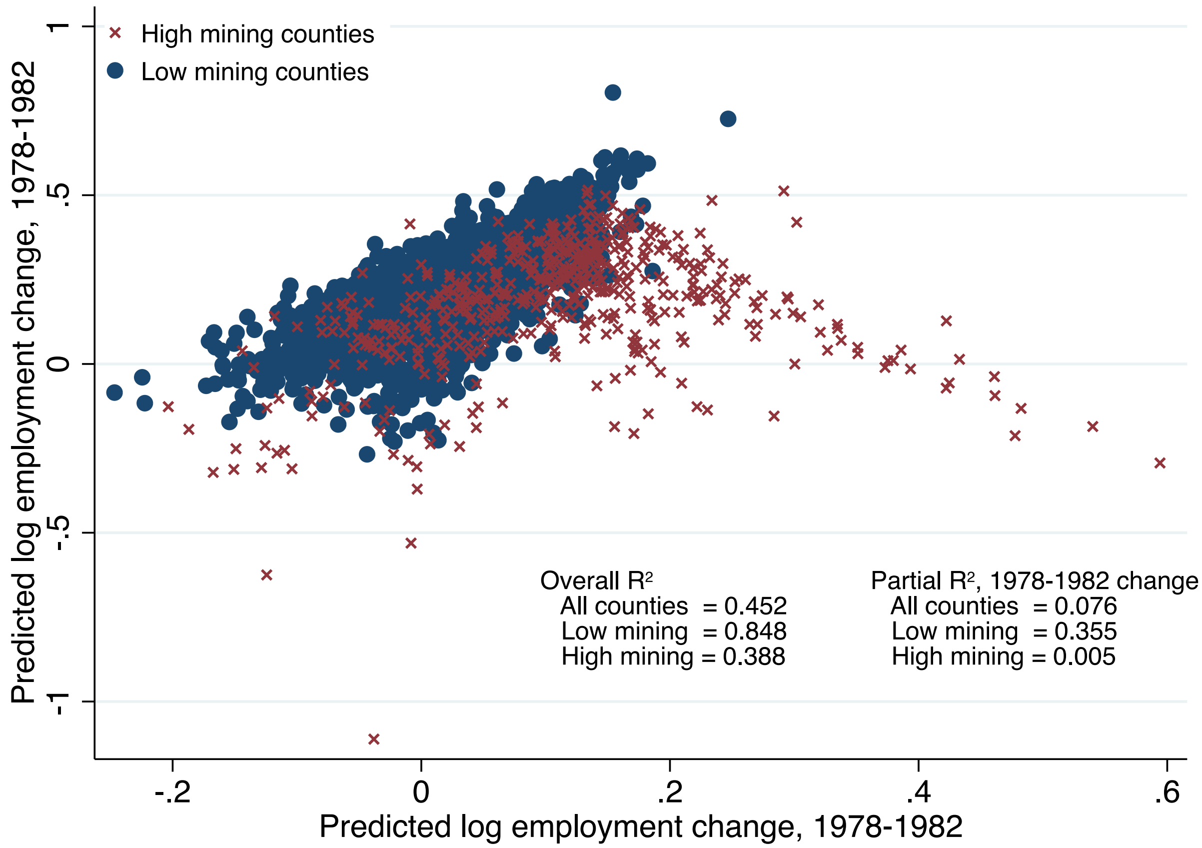
Notes: See notes to Figure 1.6. The dependent variable is an indicator for any college degree attainment.
Sources: BEA Regional Economic Accounts, Census County Business Patterns, Confidential 2000-2013 Census/ACS data linked to the SSA NUMIDENT file

Figure A.19: The Long-Run Effects of the 1980-1982 Recession on Two-Year College Degree Attainment



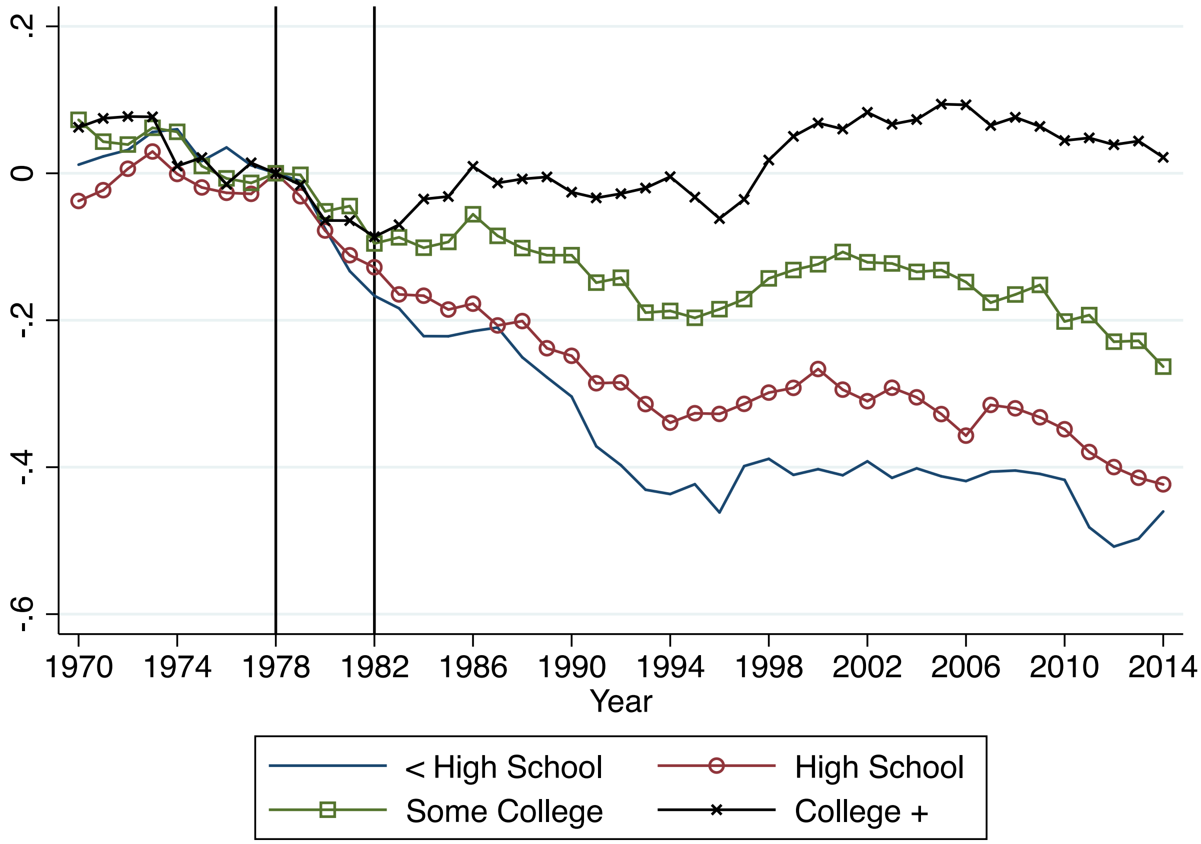
Notes: See notes to Figure 1.6. The dependent variable is an indicator for two-year college degree attainment (exactly). Sources: BEA Regional Economic Accounts, Census County Business Patterns, Confidential 2000-2013 Census/ACS data linked to the SSA NUMIDENT file

Figure A.20: Predicted Log Employment Change, 1978-1992 and Predicted Log Employment Change, 1978-1982



Notes: Predicted log employment change from 1978-1992 and 1978-1982 are constructed using a county's 1976 industrial structure and the change in industry-level employment from 1978-1992 and 1978-1982 in other states within the same region, as defined in equation (1.1). The overall R^2 includes the variation explained by state fixed effects. Overall sample contains 3,076 counties in the continental U.S. Low mining counties are the 2,550 counties with less than 5 percent of 1976 employment in the mining sector.
Source: Census County Business Patterns

Figure A.21: Normalized Median Log Real Hourly Wage of Men Age 25-54, by Education Level



Notes: Sample contains men age 25-54 who are not in the armed forces. Hourly wage is constructed as annual wage and salary income divided by the product of weeks worked last year and hours worked last week. Each line is normalized to equal 0 in 1978.
Source: March CPS

APPENDIX B

Appendix to Chapter 2

B.1 Derivation of Social Interactions Index

Appendix B.1 derives the expression for the social interactions (SI) index in equation (C.16).

First, recall the definition of the SI index, $\Delta_{j,k} \equiv \mathbb{E}[N_{-i,j,k}|D_{i,j,k} = 1] - \mathbb{E}[N_{-i,j,k}|D_{i,j,k} = 0]$. We do not distinguish among migrants within each town, implying

$$\Delta_{j,k} = (N_j - 1)\left(\mathbb{E}[D_{i',j,k}|D_{i,j,k} = 1] - \mathbb{E}[D_{i',j,k}|D_{i,j,k} = 0]\right),\ i \neq i'. \tag{B.1}$$

The law of iterated expectations implies that the probability of moving from birth town g to destination k can be written

$$P_{g,k} = \mathbb{E}[D_{i',j,k}|D_{i,j,k} = 1]P_{g,k} + \mathbb{E}[D_{i',j,k}|D_{i,j,k} = 0](1 - P_{g,k}). \tag{B.2}$$

Using the definition $\mu_{j,k} \equiv \mathbb{E}[D_{i',j,k}|D_{i,j,k} = 1]$ and rearranging equation (B.2) yields

$$\mathbb{E}[D_{i',j,k}|D_{i,j,k} = 0] = \frac{P_{g,k}(1 - \mu_{j,k})}{1 - P_{g,k}}. \tag{B.3}$$

Hence, we have

$$\mathbb{E}[D_{i',j,k}|D_{i,j,k}=1] - \mathbb{E}[D_{i',j,k}|D_{i,j,k}=0] = \mu_{j,k} - \frac{P_{g,k}(1-\mu_{j,k})}{1-P_{g,k}} \tag{B.4}$$

$$= \frac{\mu_{j,k}-P_{g,k}}{1-P_{g,k}}. \tag{B.5}$$

Substituting equation (B.5) into equation (B.1) yields

$$\Delta_{j,k} = (N_j - 1)\left(\frac{\mu_{j,k}-P_{g,k}}{1-P_{g,k}}\right). \tag{B.6}$$

Applying the law of iterated expectations to the first term of the covariance of location decisions, $C_{j,k}$, yields

$$C_{j,k} \equiv \mathbb{E}[D_{i',j,k}D_{i,j,k}] - \mathbb{E}[D_{i',j,k}]\,\mathbb{E}[D_{i,j,k}] \tag{B.7}$$

$$= \mathbb{E}[D_{i',j,k}|D_{i,j,k}=1]P_{g,k} - (P_{g,k})^2 \tag{B.8}$$

Using the definition of $\mu_{j,k}$ and rearranging yields $\mu_{j,k} - P_{g,k} = C_{j,k}/P_{g,k}$. Substituting this expression into (B.6) yields equation (C.16).

B.2 Method of Moments Formulation

B.2.1 Basic Model

As described in the text, we can derive the destination level SI index, Δ_k, in two ways: as a weighted sum of birth town-specific SI indices, $\Delta_{j,k}$, or by assuming that the SI index is constant across birth towns within a birth state. Both approaches lead to the same point estimate of the destination level SI index, but the latter approach allows us to use the method of moments to estimate standard errors.

If we assume that the SI index, $\Delta_{j,k}$, is constant across birth towns within a birth state, the destination level SI index, Δ_k, can be written

$$\Delta_k = \Delta_{j,k} = \frac{C_{j,k}(N_j - 1)}{P_{j,k} - P_{j,k}^2}. \tag{B.9}$$

It is useful to rewrite this as

$$\Delta_k \left(P_{j,k} - P_{j,k}^2\right) - C_{j,k}(N_j - 1) = 0. \tag{B.10}$$

To conduct inference, we treat the birth town group as the level of observation. Aggregating across towns within a birth town group yields

$$\Delta_k Y_{g,k} - X_{g,k} = 0, \tag{B.11}$$

where

$$X_{g,k} \equiv \sum_{j \in g} C_{j,k}(N_j - 1) \tag{B.12}$$

$$Y_{g,k} \equiv \sum_{j \in g} P_{j,k} - P_{j,k}^2. \tag{B.13}$$

In the text, we describe how we construct our estimates $\widehat{P_{j,k}}, \widehat{P_{j,k}^2}$, and $\widehat{C_{j,k}}$. These estimates immediately lead to estimates $\widehat{X_{g,k}}$ and $\widehat{Y_{g,k}}$, which can be written as deviations from the underlying parameters,

$$\widehat{X_{g,k}} = X_{g,k} + u_{g,k}^X \tag{B.14}$$

$$\widehat{Y_{g,k}} = Y_{g,k} + u_{g,k}^Y. \tag{B.15}$$

This allows us to rewrite equation (B.11),

$$\Delta_k \widehat{Y_{g,k}} - \widehat{X_{g,k}} + (\Delta_k u_{g,k}^Y - u_{g,k}^X) = 0. \tag{B.16}$$

Because we have unbiased estimators of $P_{j,k}$, $P^2_{j,k}$, and $C_{j,k}$, we have unbiased estimators of $X_{g,k}$ and $Y_{g,k}$. This implies that

$$\mathbb{E}\left[\Delta_k \widehat{Y_{g,k}} - \widehat{X_{g,k}}\right] = 0. \tag{B.17}$$

Equation (B.17) is the basis of our method of moments estimator. The sample analog is

$$\frac{1}{G}\sum_g \left(\widehat{\Delta_k}\widehat{Y_{g,k}} - \widehat{X_{g,k}}\right) = 0, \tag{B.18}$$

where G is the number of birth town groups in a state. This can be rewritten

$$\widehat{\Delta_k} = \frac{\sum_j \widehat{C_{j,k}}(N_j - 1)}{\sum_{j'} \widehat{P_{j',k}} - \widehat{P^2_{j',k}}}. \tag{B.19}$$

Equation (B.19) is identical to equation (2.9).

The above derivation is for a single destination level SI index parameter, but can easily be expanded to consider all K destination level SI index parameters. The aggregated moment condition is

$$\mathbb{E}\begin{bmatrix} \Delta_1\widehat{Y_{g,1}} - \widehat{X_{g,1}} \\ \vdots \\ \Delta_K\widehat{Y_{g,K}} - \widehat{X_{g,K}} \end{bmatrix} \equiv \mathbb{E}\left[\boldsymbol{f}(\boldsymbol{w}_g, \boldsymbol{\Delta})\right] = \boldsymbol{0} \tag{B.20}$$

where $\boldsymbol{w}_g$ is observed data used to construct $\widehat{\boldsymbol{X_g}}$ and $\widehat{\boldsymbol{Y_g}}$.

Let $\boldsymbol{\Delta} \equiv (\Delta_1, \ldots, \Delta_K)'$ be a $K \times 1$ vector of destination level SI index parameters. Under standard conditions (e.g., Cameron and Trivedi, 2005), the asymptotic distribution is

$$\sqrt{G}(\hat{\boldsymbol{\Delta}} - \boldsymbol{\Delta}) \xrightarrow{d} \mathcal{N}\left[\boldsymbol{0}, \hat{\boldsymbol{F}}^{-1}\hat{\boldsymbol{S}}(\hat{\boldsymbol{F}}')^{-1}\right], \tag{B.21}$$

where

$$\hat{\boldsymbol{F}} = \frac{1}{G}\sum_g \left.\frac{\partial \boldsymbol{f}_g}{\partial \boldsymbol{\Delta}'}\right|_{\hat{\boldsymbol{\Delta}}} \tag{B.22}$$

$$= \frac{1}{G}\sum_g \begin{bmatrix} \widehat{Y_{g,1}} & 0 & 0 & \cdots & 0 \\ 0 & \widehat{Y_{g,2}} & 0 & \cdots & 0 \\ \vdots & \vdots & \vdots & \vdots & \vdots \\ 0 & 0 & \cdots & \cdots & \widehat{Y_{g,K}} \end{bmatrix} \tag{B.23}$$

and

$$\hat{\boldsymbol{S}} = \frac{1}{G}\sum_g \boldsymbol{f}(\boldsymbol{W}_g, \hat{\boldsymbol{\Delta}})\boldsymbol{f}(\boldsymbol{W}_g, \hat{\boldsymbol{\Delta}})'. \tag{B.24}$$

While it is convenient to describe the asymptotic properties when grouping all destinations together into $\boldsymbol{\Delta}$, each destination level SI index parameter Δ_k is estimated independently of the other estimates.

B.2.2 Comparing Estimates from Two Models

The method of moments framework facilitates a comparison of estimates from different models. Under the null hypothesis we wish to test, we have two unbiased estimates for $X_{g,k}$ and $Y_{g,k}$:

$$\widehat{X^1_{g,k}} = X_{g,k} + u^X_{g,k} \tag{B.25}$$

$$\widehat{Y^1_{g,k}} = Y_{g,k} + u^Y_{g,k} \tag{B.26}$$

$$\widehat{X^2_{g,k}} = X_{g,k} + v^X_{g,k} \tag{B.27}$$

$$\widehat{Y^2_{g,k}} = Y_{g,k} + v^Y_{g,k} \tag{B.28}$$

We estimate the unrestricted version of the model using the method of moments, for which the sample analog of the moment condition is

$$\frac{1}{G}\sum_{g}\begin{pmatrix}\Delta_k^1\widehat{Y_{g,k}^1}-\widehat{X_{g,k}^1}\\ \Delta_k^2\widehat{Y_{g,k}^2}-\widehat{X_{g,k}^2}\end{pmatrix} \tag{B.29}$$

We simply stack the two estimates of the destination level SI index, Δ_k into a single, exactly-identified system.

Let $\Delta^1 \equiv N^{-1}\sum_k N_k\Delta_k$ be the migrant-weighted average of the destination level SI index parameters, where $N \equiv \sum_k N_k$ is the total number of migrants from a birth state. We are interested in testing whether $\Delta^1 = \Delta^2$. To test this hypothesis, we form the test statistic

$$\hat{t} = \frac{\widehat{\Delta^1}-\widehat{\Delta^2}}{\left(\widehat{\mathbb{V}}[\widehat{\Delta^1}-\widehat{\Delta^2}]\right)^{1/2}} \tag{B.30}$$

Given destination level SI index estimates $\widehat{\Delta_k^1}$ and $\widehat{\Delta_k^2}$, it is straightforward to construct the averages $\widehat{\Delta^1}$ and $\widehat{\Delta^2}$. To estimate the variance in the denominator of the test statistic, we assume that destination level SI index estimates are independent of each other. Given the large number of sending birth towns, and the large number of destinations, we believe that the covariance between two destination level social interaction estimates is likely small. Furthermore, we are not confident in our ability to reliably estimate the covariance of the covariances of location decisions, as would be necessary if we did not assume independence. Under the independence assumption, we can estimate $\widehat{\mathbb{V}}[\widehat{\Delta^1}-\widehat{\Delta^2}]$ as the appropriately weighted sum of

$$\widehat{\mathbb{V}}[\widehat{\Delta_k^1}-\widehat{\Delta_k^2}] = \widehat{\mathbb{V}}[\widehat{\Delta_k^1}]+\widehat{\mathbb{V}}[\widehat{\Delta_k^2}]-2\widehat{\mathbb{C}}[\widehat{\Delta_k^1},\widehat{\Delta_k^2}] \tag{B.31}$$

which we obtain from the method of moments variance estimate.

B.3 Estimating Cross-Group Social Interactions

Appendix B.3 discusses the estimation procedure and results for social interactions across different groups of migrants.

Consider the average number of people of type b induced to move from birth town j to destination county k when a randomly chosen person of type w makes the same move,

$$\Delta_{j,k}^{b|w} \equiv \mathbb{E}[N_{j,k}^{b}|D_{i,j,k}^{w}=1]-\mathbb{E}[N_{j,k}^{b}|D_{i,j,k}^{w}=0]. \tag{B.32}$$

The steps described in Appendix B.1 yield

$$\Delta_{j,k}^{b|w}=\frac{C_{j,k}^{b,w}N_{j}^{b}}{P_{j,k}^{w}(1-P_{j,k}^{w})}, \tag{B.33}$$

where $C_{j,k}^{b,w}$ is the covariance of location decisions between migrants of type b and w, N_j^b is the number of type b migrants born in j, and $P_{j,k}^{w}$ is the probability that a migrant of type w moves from j to k.

We estimate $P_{j,k}^{w}$ as described in the text. To estimate $C_{j,k}^{b,w}$, consider the model

$$D_{i,j(i),k}^{b}\cdot D_{i',j(i'),k}^{w}=\alpha_{g,k}+\sum_{j\in g}\beta_{j,k}^{b,w}1[j(i)=j(i')=j]+\epsilon_{i,i',k}. \tag{B.34}$$

This model is analogous to equation (2.2) in the text and yields the following covariance estimator,

$$\hat{C}_{j,k}^{b,w}=\frac{N_{j,k}^{b}N_{j,k}^{w}}{N_{j}^{b}N_{j}^{w}}-\frac{\sum_{j\in g}\sum_{j'\neq j\in g}N_{j,k}^{b}N_{j',k}^{w}}{\sum_{j\in g}\sum_{j'\neq j\in g}N_{j}^{b}N_{j'}^{w}}. \tag{B.35}$$

We estimate destination level social interaction parameters as

$$\hat{\Delta}_{k}^{b|w}=\sum_{j}\left(\frac{\hat{P}_{j,k}^{w}(1-\hat{P}_{j,k}^{w})}{\sum_{j'}\hat{P}_{j',k}^{w}(1-\hat{P}_{j',k}^{w})}\right)\hat{\Delta}_{j,k}^{b|w}. \tag{B.36}$$

We only estimate social interactions for destinations which received at least ten black and white migrants from a given state. When calculating weighted averages of $\hat{\Delta}_{k}^{b|w}$, we use the number of type w individuals who moved to each destination.

Panel A of appendix table B.6 reports estimates of the average number of Southern black migrants induced to move from birth town j to destination county k when a randomly chosen Southern white makes the same move. Our preferred specification in column 4 excludes the largest CMSAs. Weighted averages are small and/or indistinguishable from zero, varying from -0.079 (0.084) in Florida to 0.391 (0.260) in Alabama. Panel B reports estimates of the average number of white migrants induced to move from j to k when a randomly chosen black migrant makes the same move. When excluding the largest CMSAs, we find little evidence that Southern whites co-located with black migrants. The lack of social influence between black and white migrants is consistent with the segregation of the Jim Crow South.

B.4 Additional Detail on Measurement Error due to Incomplete Migration Data

This section discusses the implications of measurement error due to incomplete migration data without making a missing at random (MAR) assumption. We derive a lower bound on the social interactions (SI) index and show that estimates of this lower bound still reveal sizable social interactions.

As described in the text, the SI index, $\Delta_{j,k}$, depends on the covariance of location decisions for migrants from birth town j to destination k, $C_{j,k}$, the probability of moving from birth town group g to destination k, $P_{g,k}$, and the number of migrants from town j, N_j. To focus on the key issues, we assume that the moving probability is measured accurately and consider the consequences of measurement error in the covariance of location decisions and the number of migrants. Let $\Delta^*_{j,k}, C^*_{j,k}$, and N^*_j be the true values of the SI index, covariance of location decisions, and number of migrants. The true parameters are connected through the equation

$$\Delta^*_{j,k} = \frac{C^*_{j,k}(N^*_j - 1)}{P_{g,k} - P^2_{g,k}}. \tag{B.37}$$

As in the text, we let α denote the coverage rate, defined by the relationship between the observed number of migrants, N_j, and the true number of migrants,

$$N_j = \alpha N_j^*. \tag{B.38}$$

Using the definition of the covariance of location decisions, it is straightforward to show that

$$C_{j,k}^* = \alpha^2 C_{j,k} + 2\alpha(1-\alpha) C_{j,k}^{\text{in, out}} + (1-\alpha)^2 C_{j,k}^{\text{out, out}}, \tag{B.39}$$

where $C_{j,k}$ is the covariance of location decisions between migrants who are covered by our data, $C_{j,k}^{\text{in, out}}$ is the covariance of location decisions between a migrant who is covered by our data ("in") and a migrant who is not ("out"), and $C_{j,k}^{\text{out, out}}$ is the covariance of location decisions between migrants who are not covered by our data.

When not assuming that data are MAR, the covariance of location decisions among migrants not in our data ($C_{j,k}^{\text{in, out}}$ and $C_{j,k}^{\text{out, out}}$) could differ from the covariance of location decisions between migrants who are in our data ($C_{j,k}$). As a result, the SI index based on our data, $\Delta_{j,k}$, might not simply be attenuated, as implied by the MAR assumption. In general, we cannot point identify the SI index under this more general measurement error model. However, we can construct a lower bound for the strength of social interactions. In particular, we make the extreme assumptions that there are no social interactions between migrants in and out of our sample, so that $C_{j,k}^{\text{in, out}} = 0$, and that there are no social interactions between migrants out of our sample, so that $C_{j,k}^{\text{out, out}} = 0$. In this case, equations (B.37), (B.38), and (B.39) imply that

$$\Delta_{j,k}^* \geq \alpha \Delta_{j,k}, \tag{B.40}$$

so that we can estimate a lower bound on the true SI index by multiplying the estimated SI index by the coverage rate.[1] The average coverage rate is 52.5% for African American migrants from the South and 69.7% for white migrants from the Great Plains. Combined with the average destination level SI index estimates from Table 2.3, we estimate a lower bound for the SI index of 1.017 for African Americans and 0.265 for whites. These lower bounds, which depend on extremely conservative assumptions about the migration behavior of individuals not in our sample, still reveal sizable social interactions, especially among African Americans.

B.5 A Richer Model of Local Social Interactions

This section extends the local social interactions model in Section 2.4.5. In particular, we allow the probability that a migrant follows his neighbor to vary with birth town and destination.

We categorize preferences of individual i so that each destination k belongs in one and only one of three preference groups: high (H_i), medium (M_i), or low (L_i). The high preference group is non-empty and contains a single destination. In the absence of social interactions, the destination in H_i is most preferred, while destinations in M_i are preferred relative to those in L_i.[2] An individual never moves to a place in L_i. A migrant chooses a destination in M_i if and only if his neighbor also chose the same location. An individual chooses a location in H_i if his neighbor chose the same location or his neighbor selected a destination in L_i.

The probability that k is in the high preference group for a migrant from town j is $h_{j,k} \equiv \mathbb{P}[k \in H_i | i \in j]$. Similarly, let $m_{j,k} \equiv \mathbb{P}[k \in M_i | i \in j]$. The probability that a migrant moves to k, conditional on k not being in the high preference group, is $\nu_{j,k} \equiv \mathbb{P}[k \in M_i | k \notin H_i, i \in j]$. Using

[1]Proof: If $C^{\text{in, out}}_{j,k} = C^{\text{out, out}}_{j,k} = 0$, equations (B.37), (B.38), and (B.39) imply

$$\begin{aligned}\Delta^*_{j,k} &= \frac{\alpha^2 C_{j,k}\left(\frac{N_j}{\alpha} - 1\right)}{P_{g,k} - P^2_{g,k}} \\ &\geq \frac{\alpha^2 C_{j,k}\left(\frac{N_j}{\alpha} - \frac{1}{\alpha}\right)}{P_{g,k} - P^2_{g,k}} = \alpha \Delta_{j,k},\end{aligned}$$

where the inequality comes from noting that $\alpha \in [0, 1]$ and assuming $C_{j,k} \geq 0$, and the final equality comes from equation (C.16) in the text. One could also construct upper bounds, but these are not particularly informative.

[2]The assumption that H_i is a non-empty singleton ensures that person i has a well-defined location decision in the absence of social interactions. We could relax the model to allow H_i to contain many destinations and specify a decision rule among the elements of H_i. This extension complicates the model without adding any new insights.

the conditional probability definition for $\nu_{j,k}$, it is straightforward to show that $m_{j,k} = \nu_{j,k}(1-h_{j,k})$.

The probability that i moves to k given that his neighbor moves to k is

$$\mathbb{P}[D_{i,j,k} = 1 | D_{i-1,j,k} = 1] = \mathbb{P}[k \in H_i] + \mathbb{P}[k \in M_i] \tag{B.41}$$

$$= h_{j,k} + \nu_{j,k}(1 - h_{j,k}),\ i = 2, \ldots, N_j. \tag{B.42}$$

In equilibrium, we have

$$P_{j,k} \equiv \mathbb{P}[D_{i,j,k} = 1] = \mathbb{P}[D_{i-1,j,k} = 1, k \in H_i] + \mathbb{P}[D_{i-1,j,k} = 1, k \in M_i]$$

$$+ \mathbb{P}[D_{i-1,j,k} = 0, k \in H_i, k_{i-1} \notin M_i] \tag{B.43}$$

$$= P_{j,k}h_{j,k} + P_{j,k}\nu_{j,k}(1 - h_{j,k}) + \sum_{k' \neq k} P_{j,k'}h_{j,k}(1 - \nu_{j,k'}) \tag{B.44}$$

$$= P_{j,k}\nu_{j,k} + \left(\sum_{k'=1}^{K} P_{j,k'}(1 - \nu_{j,k'}) \right) h_{j,k}, \tag{B.45}$$

where k_{i-1} denotes the choice of i's neighbor. The first term on the right hand side of equation (C.11) is the probability that an individual's neighbor moves to k, and k is in the high preference group; social interaction reinforces the migrant's desire to move to k. The second term is the probability that a migrant follows his neighbor to k because of social interactions. The third term is the probability that a migrant resists the pull of social interactions because town k offers high inherent utility and the neighbor's chosen destination offers low utility.

We now propose an estimation strategy. Recall that in the simple model, $\mathbb{P}[D_{i,j,k} = 1 | D_{i-1,j,k} = 1] = \chi + (1-\chi)P_{j,k}$. Letting $\rho_{j,k} \equiv \mathbb{P}[D_{i,j,k} = 1 | D_{i-1,j,k} = 1]$, we have $\chi = (\rho_{j,k} - P_{j,k})/(1-P_{j,k})$. The model's prediction of the average covariance is

$$C_{j,k}(P_j, \nu_j) = \frac{2P_{j,k}(1 - P_{j,k}) \sum_{s=1}^{N_j-1}(N_j - s) \left(\frac{\rho_{j,k} - P_{j,k}}{1 - P_{j,k}} \right)^s}{N_j(N_j - 1)}, \tag{B.46}$$

where $(P_j, \nu_j) \equiv ((P_{j,1}, \ldots, P_{j,K}), (\nu_{j,1}, \ldots, \nu_{j,K}))$. The same steps in the main text yield

$$\hat{\Delta}_{j,k} = \frac{2(\hat{\rho}_{j,k} - \hat{P}_{j,k})}{1 - \hat{\rho}_{j,k}}, \tag{B.47}$$

which can be used to obtain an estimate $\hat{\rho}_{j,k}$ given $(\hat{\Delta}_{j,k}, \hat{P}_{j,k})$. Note that equation (C.13) implies

$$\rho_{j,k} = \nu_{j,k} + \frac{P_{j,k}(1 - \nu_{j,k})^2}{\sum_{k'=1}^{K} P_{j,k'}(1 - \nu_{j,k'})}. \tag{B.48}$$

There are $J \cdot K$ equations of the form (C.20), which yield a GMM estimator of the $J \cdot K$ parameters in ν_j after plugging in estimates $(\hat{P}_{j,k}, \hat{\rho}_{j,k})$. Finally, equation (C.10) implies that $h_{j,k} = (\rho_{j,k} - \nu_{j,k})/(1 - \nu_{j,k})$, so that we can estimate $h_{j,k}$ using $(\hat{\rho}_{j,k}, \hat{\nu}_{j,k})$. One could reduce the number of reported parameters by imposing restrictions (e.g., assuming that $\nu_{j,k}$ is constant over some j).

Table B.1: Number of Birth Towns and Migrants per State

Birth State	Birth Towns (1)	Migrants (2)	Migrants Per Town (3)
Panel A: Black Moves out of South			
Alabama	693	96,269	138.9
Florida	203	19,158	94.4
Georgia	566	77,038	136.1
Louisiana	460	55,974	121.7
Mississippi	660	120,454	182.5
North Carolina	586	78,420	133.8
South Carolina	461	69,399	150.5
All States	3,629	516,712	142.4
Panel B: White Moves out of Great Plains			
Kansas	883	139,374	157.8
Nebraska	643	134,011	208.4
North Dakota	592	92,205	155.8
Oklahoma	966	200,392	207.4
South Dakota	474	78,541	165.7
All States	3,558	644,523	181.1

Notes: Table B.1 shows counts for all towns with at least 10 migrants in the data.
Source: Authors' calculations using Duke SSA/Medicare data

Table B.2: Average Destination Level Social Interactions Index Estimates, Birth Town Groups Defined by Cross Validation and Counties

	Cross Validation		Counties	
Type of Average:	Unweighted	Weighted	Unweighted	Weighted
Birth State	(1)	(2)	(3)	(4)
Panel A: Black Moves out of South				
Alabama	0.770	1.888	0.616	1.393
	(0.049)	(0.195)	(0.034)	(0.170)
Florida	0.536	0.813	0.597	0.811
	(0.052)	(0.117)	(0.087)	(0.317)
Georgia	0.735	1.657	0.544	0.887
	(0.048)	(0.177)	(0.039)	(0.279)
Louisiana	0.462	1.723	0.399	2.209
	(0.039)	(0.478)	(0.039)	(0.920)
Mississippi	0.901	2.303	0.742	2.166
	(0.050)	(0.313)	(0.051)	(0.401)
North Carolina	0.566	1.539	0.402	1.022
	(0.039)	(0.130)	(0.028)	(0.123)
South Carolina	0.874	2.618	0.774	2.132
	(0.054)	(0.301)	(0.049)	(0.224)
All States	0.736	1.938	0.599	1.608
	(0.020)	(0.110)	(0.017)	(0.151)
Panel B: White Moves out of Great Plains				
Kansas	0.128	0.255	0.106	0.194
	(0.007)	(0.024)	(0.008)	(0.028)
North Dakota	0.174	0.464	0.156	0.385
	(0.012)	(0.036)	(0.010)	(0.029)
Nebraska	0.141	0.361	0.121	0.399
	(0.008)	(0.082)	(0.009)	(0.117)
Oklahoma	0.112	0.453	0.102	0.372
	(0.008)	(0.036)	(0.007)	(0.036)
South Dakota	0.163	0.350	0.135	0.273
	(0.009)	(0.026)	(0.008)	(0.027)
All States	0.137	0.380	0.119	0.329
	(0.004)	(0.022)	(0.004)	(0.028)

Notes: Column 1 is an unweighted average of destination level social interaction estimates, $\hat{\Delta}_k$. Column 2 is a weighted average, where the weights are the number of people who move from each state to destination k. Birth town groups are defined by counties. Standard errors in parentheses.
Source: Authors' calculations using Duke SSA/Medicare data

Table B.3: Average Social Interactions Index Estimates, White Moves out of South

	Number	Type of Average	
Birth State	of Migrants (1)	Unweighted (2)	Weighted (3)
Alabama	43,157	0.204	0.516
		(0.014)	(0.052)
Florida	27,426	0.046	0.072
		(0.006)	(0.100)
Georgia	31,299	0.082	0.117
		(0.007)	(0.021)
Louisiana	31,303	0.122	0.269
		(0.011)	(0.071)
Mississippi	28,001	0.118	0.186
		(0.010)	(0.021)
North Carolina	47,146	0.179	0.412
		(0.012)	(0.040)
South Carolina	14,605	0.068	0.094
		(0.005)	(0.029)
All States	222,937	0.131	0.280
		(0.004)	(0.021)

Notes: Column 2 is an unweighted av***erage of destination level social interaction estimates, $\hat{\Delta}_k$. Column 3 is a weighted average, where the weights are the number of people who move from each state to destination k. Birth town groups are defined by cross validation. Standard errors in parentheses.
Source: Authors' calculations using Duke SSA/Medicare data

Table B.4: Average Social Interactions Index Estimates, By Size of Birth Town and Destination, White Moves out of South

Exclude Largest Birth Towns Exclude Largest Destinations	No No	Yes No	No Yes	Yes Yes
Birth State	(1)	(2)	(3)	(4)
Alabama	0.516	0.458	0.531	0.481
	(0.052)	(0.045)	(0.071)	(0.062)
Florida	0.072	0.074	0.134	0.030
	(0.100)	(0.012)	(0.082)	(0.009)
Georgia	0.117	0.101	0.119	0.088
	(0.021)	(0.012)	(0.019)	(0.013)
Louisiana	0.269	0.207	0.198	0.143
	(0.071)	(0.022)	(0.035)	(0.017)
Mississippi	0.186	0.185	0.135	0.134
	(0.021)	(0.022)	(0.013)	(0.013)
North Carolina	0.412	0.395	0.337	0.319
	(0.040)	(0.037)	(0.040)	(0.034)
South Carolina	0.094	0.090	0.058	0.055
	(0.029)	(0.023)	(0.013)	(0.012)
All States	0.280	0.254	0.262	0.223
	(0.021)	(0.013)	(0.021)	(0.015)

Notes: Column 1 is a weighted average of destination level social interaction estimates, $\hat{\Delta}_k$, where the weights are the number of people who move from each state to destination k. In column 2, we exclude birth towns with 1920 population greater than 20,000 when estimating each $\hat{\Delta}_k$. In column 3, we exclude all counties which intersect in 2000 with the ten largest non-South CMSAs as of 1950: New York, Chicago, Los Angeles, Philadelphia, Boston, Detroit, Washington D.C., San Francisco, Pittsburgh, and St. Louis, in addition to counties which receive fewer than 10 migrants. Column 4 excludes large birth towns and large destinations. Birth town groups are defined by cross validation. Standard errors in parentheses.
Source: Authors' calculations using Duke SSA/Medicare data

Table B.5: Average Social Interactions Index Estimates, by Destination Region, White Moves out of South

	Destination Region			
	Northeast (1)	Midwest (2)	West (3)	South (4)
Alabama	0.140	1.048	0.208	-
	(0.021)	(0.123)	(0.034)	-
Florida	0.090	0.070	0.277	-
	(0.017)	(0.020)	(0.104)	-
Georgia	0.104	0.307	0.082	-
	(0.013)	(0.049)	(0.023)	-
Louisiana	0.159	0.450	0.331	-
	(0.027)	(0.100)	(0.100)	-
Mississippi	0.067	0.301	0.127	-
	(0.014)	(0.052)	(0.014)	-
North Carolina	0.549	0.489	0.302	-
	(0.063)	(0.122)	(0.048)	-
South Carolina	0.111	0.081	0.073	-
	(0.011)	(0.012)	(0.022)	-
All States	0.275	0.534	0.220	-
	(0.024)	(0.044)	(0.026)	-

Notes: All columns contain weighted averages of social interaction estimates, $\hat{\Delta}_k$, where the weights are the number of people who move from each state to destination k. See footnote 36 for region definitions. We do not estimate social interactions for blacks which move to the South. Birth town groups are defined by cross validation. Standard errors in parentheses.
Source: Authors' calculations using Duke SSA/Medicare data

Table B.6: Average Cross-Race Social Interactions Index Estimates, Southern White and Black Migrants

Birth State	All Counties (1)	Excluding Largest CMSAs (2)
Panel A: Blacks Induced to Location by Randomly Chosen White Migrant		
Alabama	0.188	0.130
	(0.106)	(0.150)
Florida	0.026	0.005
	(0.059)	(0.036)
Georgia	-0.028	0.040
	(0.039)	(0.044)
Louisiana	-0.066	0.068
	(0.196)	(0.038)
Mississippi	0.246	0.049
	(0.185)	(0.033)
North Carolina	-0.010	-0.005
	(0.062)	(0.011)
South Carolina	0.197	-0.025
	(0.161)	(0.027)
All States	0.071	0.050
	(0.048)	(0.033)
Panel B: Whites Induced to Location by Randomly Chosen Black Migrant		
Alabama	0.052	0.038
	(0.048)	(0.042)
Florida	0.047	-0.018
	(0.064)	(0.036)
Georgia	-0.020	0.004
	(0.014)	(0.014)
Louisiana	-0.137	0.016
	(0.066)	(0.017)
Mississippi	-0.056	0.020
	(0.030)	(0.011)
North Carolina	0.021	-0.002
	(0.029)	(0.022)
South Carolina	-0.019	0.020
	(0.013)	(0.018)
All States	-0.019	0.019
	(0.015)	(0.013)

Notes: Table B.6 contains averages of cross-group social interaction estimates. See note to Table 2.3. Birth town groups are defined by cross validation. Standard errors in parentheses.
Source: Authors' calculations using Duke SSA/Medicare data

Table B.7: Fraction of Population from 1960/1970 Census in Duke Data

	Group				
Birth State	All (1)	Men (2)	Women (3)	Born 1916-25 (4)	Born 1926-36 (5)
Panel A: African Americans Born in South					
Alabama	55.4%	53.0%	57.4%	47.6%	63.3%
Florida	50.1%	51.7%	48.8%	44.5%	55.0%
Georgia	49.3%	46.5%	51.4%	43.2%	56.1%
Louisiana	57.8%	57.6%	58.0%	52.7%	62.7%
Mississippi	55.9%	56.0%	55.9%	48.2%	64.1%
North Carolina	50.3%	46.5%	53.0%	42.2%	58.6%
South Carolina	46.0%	43.2%	48.1%	38.7%	54.8%
Panel B: Whites Born in Great Plains					
Kansas	70.5%	71.2%	69.8%	66.5%	74.8%
Nebraska	69.4%	68.8%	70.0%	64.9%	74.2%
North Dakota	67.7%	64.4%	70.8%	62.9%	72.7%
Oklahoma	69.3%	67.6%	70.8%	64.4%	73.9%
South Dakota	72.5%	73.0%	72.0%	66.6%	79.2%

Notes: We use the 1960 Census for individuals born from 1916-1925 and the 1970 Census for individuals born from 1926-1936.

Source: Authors' calculations using Duke SSA/Medicare data and Ruggles et al. (2010) data

Table B.8: Weighted Averages of Destination Level Social Interactions Index Estimates, Adjusted for Coverage Rate

Birth State	All (1)	Men (2)	Women (3)	Born 1916-25 (4)	Born 1926-36 (5)
Panel A: Black Moves out of South					
Alabama	3.408	1.600	1.825	1.742	1.859
	(0.352)	(0.166)	(0.197)	(0.198)	(0.183)
Florida	1.623	0.746	0.867	0.669	1.022
	(0.234)	(0.119)	(0.175)	(0.150)	(0.161)
Georgia	3.362	1.345	2.017	2.072	1.549
	(0.359)	(0.156)	(0.240)	(0.281)	(0.142)
Louisiana	2.981	1.528	1.202	1.246	2.031
	(0.827)	(0.407)	(0.471)	(0.289)	(0.694)
Mississippi	4.119	1.813	2.342	1.850	2.393
	(0.560)	(0.252)	(0.341)	(0.279)	(0.328)
North Carolina	3.061	1.420	1.693	1.771	1.505
	(0.259)	(0.138)	(0.146)	(0.167)	(0.123)
South Carolina	5.692	2.567	3.186	3.273	2.654
	(0.654)	(0.264)	(0.439)	(0.429)	(0.278)
All States	3.739	1.678	2.066	1.994	1.978
	(0.201)	(0.090)	(0.125)	(0.115)	(0.120)
Panel B: White Moves out of Great Plains					
Kansas	0.362	0.179	0.201	0.241	0.188
	(0.034)	(0.019)	(0.019)	(0.024)	(0.015)
Nebraska	0.520	0.224	0.292	0.337	0.270
	(0.118)	(0.064)	(0.057)	(0.071)	(0.053)
North Dakota	0.685	0.318	0.366	0.457	0.320
	(0.054)	(0.027)	(0.034)	(0.038)	(0.024)
Oklahoma	0.653	0.318	0.336	0.352	0.379
	(0.052)	(0.029)	(0.027)	(0.030)	(0.031)
South Dakota	0.483	0.212	0.274	0.314	0.237
	(0.036)	(0.020)	(0.023)	(0.026)	(0.018)
All States	0.548	0.256	0.295	0.336	0.292
	(0.032)	(0.018)	(0.016)	(0.020)	(0.016)

Table B.8 contains weighted averages of destination level social interaction estimates for each cohort. Columns 1 and 2 are not adjusted for differences in undercount among each cohort. Columns 3 and 4 are adjusted using results from table B.7. See note to Table 2.3. Standard errors in parentheses.
Source: Authors' calculations using Duke SSA/Medicare data

Table B.9: Summary Statistics, Destination Characteristics

Variable	Mean	S.D.
Panel A: Black Moves out of South (N=1469)		
Social interaction estimate, $\hat{\Delta}_k$	0.732	1.373
Manufacturing employment share, 1910	0.24	0.14
Direct railroad connection	0.093	0.291
One-stop railroad connection	0.557	0.497
Log distance from birth state	6.684	0.517
Log number of migrants from birth state	4.211	1.5
Log Population, 1900	11.004	1.105
Percent African-American, 1900	0.045	0.082
Panel B: White Moves Out of South (N=3153)		
Social interaction estimate, $\hat{\Delta}_k$	0.131	0.566
Manufacturing employment share, 1910	0.195	0.141
Direct railroad connection	0.084	0.278
One-stop railroad connection	0.492	0.5
Log distance from birth state	6.766	0.593
Log number of migrants from birth state	3.453	0.961
Log Population, 1900	10.418	1.143
Percent African-American, 1900	0.038	0.077
Panel C: White Moves out of Great Plains (N=3822)		
Social interaction estimate, $\hat{\Delta}_k$	0.14	0.441
Manufacturing employment share, 1910	0.169	0.134
Direct railroad connection	0.112	0.315
One-stop railroad connection	0.504	0.5
Log distance from birth state	6.788	0.355
Log number of migrants from birth state	3.748	1.281
Log Population, 1900	10.122	1.08
Percent African-American, 1900	0.121	0.197

Notes: Sample includes destination counties which existed from 1900-2000 and for which we estimate social interactions. Birth town groups are defined by cross validation.
Sources: Duke SSA/Medicare data, Haines and ICPSR (2010) data

Table B.10: Social Interaction Estimates and Destination County Characteristics, Black Moves out of South, Groups Defined by Counties

Dependent Variable: Destination Level Social Interaction Estimate	(1)	(2)	(3)
Manufacturing employment share, 1910	1.529**	0.741**	0.710**
	(0.595)	(0.325)	(0.344)
Manufacturing employment share X		1.533**	1.516**
small destination indicator		(0.774)	(0.717)
Small destination indicator		-0.059	-0.061
		(0.165)	(0.146)
Direct railroad connection	0.168	0.151	0.124
	(0.126)	(0.131)	(0.157)
One-stop railroad connection	0.120	0.100	0.065
	(0.106)	(0.101)	(0.104)
Log distance from birth state	-0.273***	-0.220***	-0.280***
	(0.074)	(0.079)	(0.066)
Log number of migrants from birth state	0.202***	0.227***	0.233***
	(0.043)	(0.044)	(0.038)
Log Population, 1900	-0.066**	-0.046	-0.054
	(0.033)	(0.032)	(0.036)
Percent African-American, 1900	-1.604***	-1.256***	-1.348***
	(0.326)	(0.342)	(0.325)
Birth state fixed effects			x
Observations	1,469	1,469	1,469
R-squared	0.084	0.098	0.107
Clusters	371	371	371

Notes: See note to table 2.7. The sample does not include any counties which intersect with the largest cities or counties which received fewer than 10 migrants (see note to table 2.3). Standard errors, clustered by destination county, in parentheses. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

Source: Authors' calculations using Duke SSA/Medicare data and Haines and ICPSR (2010) data

Table B.11: Social Interaction Estimates and Destination County Characteristics, Whites Moves from Great Plains

Dependent Variable: Destination Level Social Interaction Estimate	(1)	(2)	(3)
Manufacturing employment share, 1910	0.025	-0.151*	-0.145*
	(0.076)	(0.077)	(0.077)
Manufacturing employment share X small destination indicator		0.226**	0.221**
		(0.111)	(0.111)
Small destination indicator		0.028	0.028
		(0.033)	(0.033)
Direct railroad connection	0.097**	0.098**	0.073
	(0.042)	(0.042)	(0.045)
One-stop railroad connection	0.037**	0.033**	0.029*
	(0.016)	(0.015)	(0.015)
Log distance from birth state	-0.064*	-0.048	-0.071*
	(0.035)	(0.035)	(0.037)
Log number of migrants from birth state	0.071***	0.072***	0.074***
	(0.009)	(0.009)	(0.010)
Log Population, 1900	0.010	0.019**	0.019**
	(0.007)	(0.008)	(0.008)
Percent African-American, 1900	-0.185***	-0.198***	-0.190***
	(0.030)	(0.032)	(0.031)
Birth state fixed effects			x
Observations	3,822	3,822	3,822
R-squared	0.066	0.070	0.072
Clusters	1148	1148	1148

Notes: See note to table 2.7. Standard errors, clustered by destination county, in parentheses.
* $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$
Source: Authors' calculations using Duke SSA/Medicare data and Haines and ICPSR (2010) data

Table B.12: Social Interaction Estimates and Destination County Characteristics, Whites Moves out of South

Dependent Variable: Destination Level Social Interaction Estimate			
	(1)	(2)	(3)
Manufacturing employment share, 1910	0.467***	0.223	0.210
	(0.163)	(0.142)	(0.141)
Manufacturing employment share X small destination indicator		0.371**	0.391**
		(0.184)	(0.187)
Small destination indicator		-0.023	-0.030
		(0.047)	(0.047)
Direct railroad connection	-0.017	-0.022	-0.042
	(0.039)	(0.039)	(0.039)
One-stop railroad connection	0.012	0.009	0.002
	(0.018)	(0.018)	(0.017)
Log distance from birth state	-0.144***	-0.142***	-0.135***
	(0.028)	(0.028)	(0.030)
Log number of migrants from birth state	0.158***	0.162***	0.159***
	(0.026)	(0.027)	(0.027)
Log Population, 1900	-0.071***	-0.064***	-0.060***
	(0.018)	(0.016)	(0.016)
Percent African-American, 1900	-0.544***	-0.518***	-0.478***
	(0.119)	(0.116)	(0.114)
Birth state fixed effects			x
Observations	3,153	3,153	3,153
R-squared	0.071	0.074	0.079
Clusters	728	728	728

Notes: See note to table 2.7. Standard errors, clustered by destination county, in parentheses. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

Source: Authors' calculations using Duke SSA/Medicare data and Haines and ICPSR (2010) data

Table B.13: Summary Statistics, Birth County Characteristics

Variable	Mean	S.D.
A: Black Moves out of South (N=551)		
Social interaction estimate, $\hat{\Delta}_c$	1.717	3.538
Share with income less than $2,000 (1950)	0.629	0.144
Percent rural, 1950	0.769	0.231
Rosenwald exposure	0.204	0.217
Railroad exposure	0.540	0.405
Percent African-American, 1920	0.407	0.209

Notes: Sample includes Southern counties containing at least one town with at least 10 migrants.

Sources: Duke SSA/Medicare data, Haines and ICPSR (2010) data

Table B.14: Estimated Share of Migrants Which Chose Their Destination Because of Social Interactions, White Moves out of South

	Destination Region				
	All	Northeast	Midwest	West	South
Birth State	(1)	(2)	(3)	(4)	(5)
Alabama	0.205	0.065	0.344	0.094	-
	(0.016)	(0.009)	(0.027)	(0.014)	-
Florida	0.035	0.043	0.034	0.122	-
	(0.047)	(0.008)	(0.009)	(0.040)	-
Georgia	0.055	0.049	0.133	0.039	-
	(0.009)	(0.006)	(0.018)	(0.010)	-
Louisiana	0.119	0.074	0.184	0.142	-
	(0.028)	(0.011)	(0.033)	(0.037)	-
Mississippi	0.085	0.032	0.131	0.060	-
	(0.009)	(0.006)	(0.020)	(0.006)	-
North Carolina	0.171	0.215	0.196	0.131	-
	(0.014)	(0.020)	(0.039)	(0.018)	-
South Carolina	0.045	0.052	0.039	0.035	-
	(0.013)	(0.005)	(0.005)	(0.010)	-
All States	0.123	0.121	0.211	0.099	-
	(0.008)	(0.009)	(0.014)	(0.010)	-

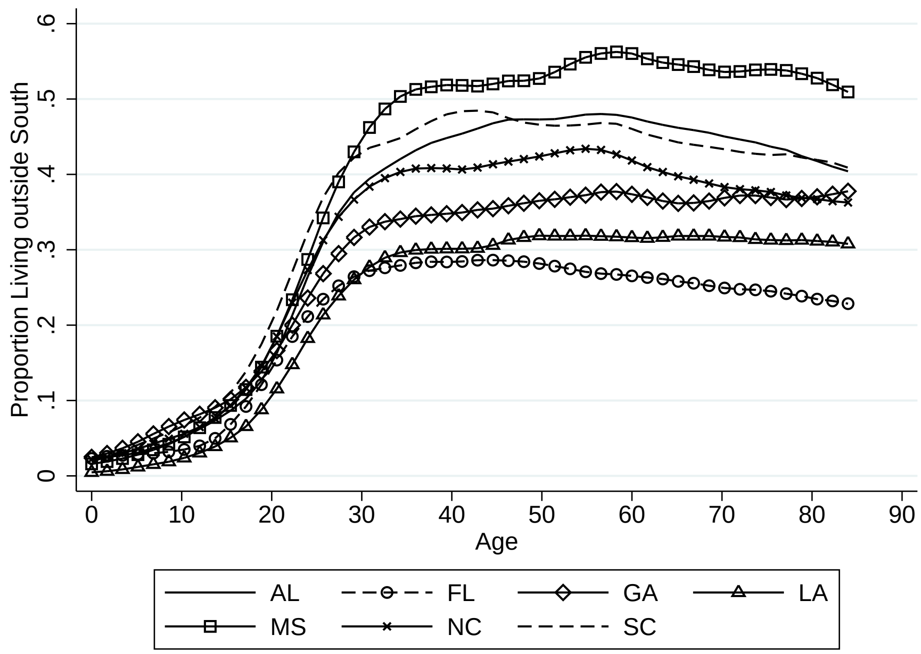
Notes: Table contains estimates and standard errors of $\chi = \Delta/(2+\Delta)$, the share of migrants which chose their destination because of social interactions, based on weighted average estimates from column 2 of table B.3 and columns 1-4 of table B.5. Standard errors, estimated using the Delta method, are in parentheses.
Source: Authors' calculations using Duke SSA/Medicare data

Table B.15: Industry of Migrants and Non-Migrants, Southern Blacks and Great Plains Whites, 1950

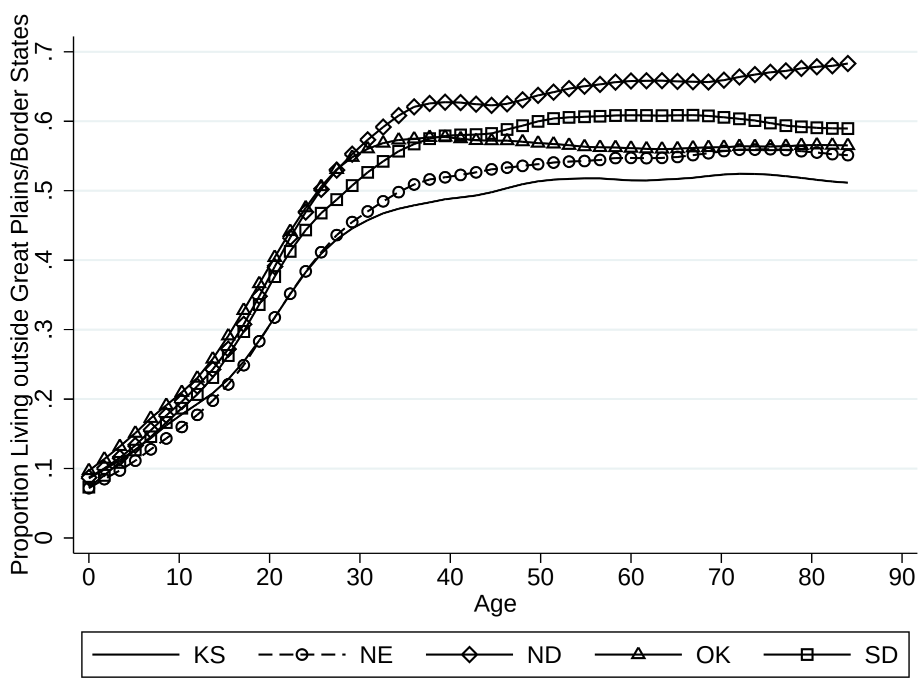
	Percent of Group Working in Industry			
	Southern Blacks		Great Plains Whites	
	Migrants (1)	Non-Migrants (2)	Migrants (3)	Non-Migrants (4)
Agriculture, Forestry, and Fishing	1.23%	35.92%	9.38%	31.60%
Mining	1.33%	1.21%	2.02%	3.65%
Construction	10.19%	8.12%	11.98%	9.14%
Manufacturing	37.87%	22.09%	23.79%	10.98%
Transportation, Communication, and Other Utilities	11.80%	7.89%	9.58%	9.59%
Wholesale and Retail Trade	13.61%	10.46%	16.47%	16.87%
Finance, Insurance, and Real Estate	2.21%	0.78%	2.39%	2.20%
Business and Repair Services	2.98%	1.67%	4.11%	3.49%
Personal Services	6.30%	5.24%	2.16%	1.83%
Entertainment and Recreation Services	1.03%	0.63%	1.15%	0.76%
Professional and Related Services	3.95%	3.31%	5.67%	4.27%
Public Administration	6.57%	2.33%	11.08%	5.17%
Other	0.92%	0.35%	0.22%	0.43%
Total count	558,538	1,265,691	638,039	1,446,053

Note: Sample contains currently employed males, age 20-60 in the 1950 Census.
Source: Ruggles et al. (2010)

Figure B.1: Proportion Living Outside Home Region, 1916-1936 Birth Cohorts, by Birth State and Age



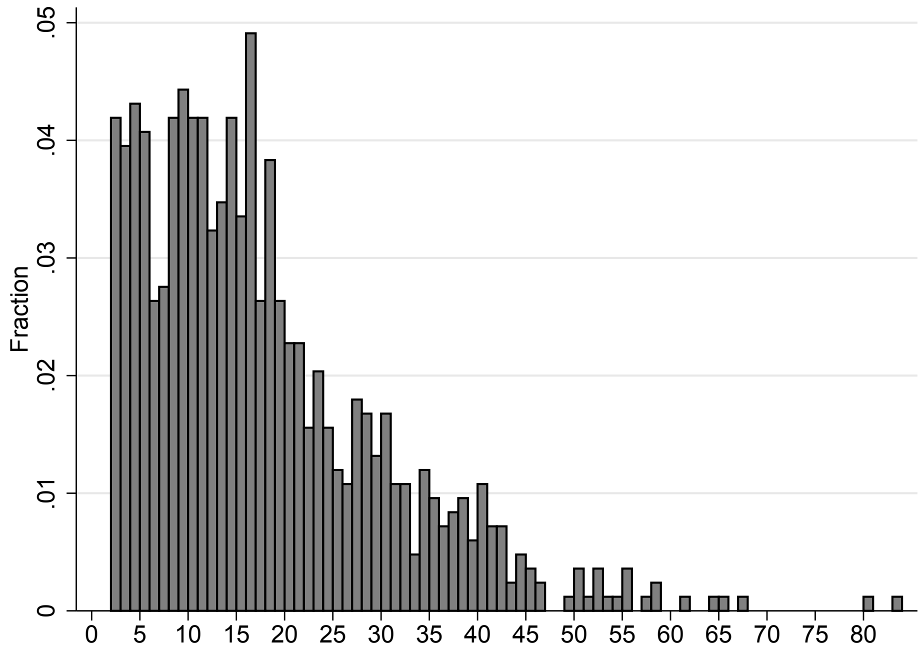
(a) Southern Blacks



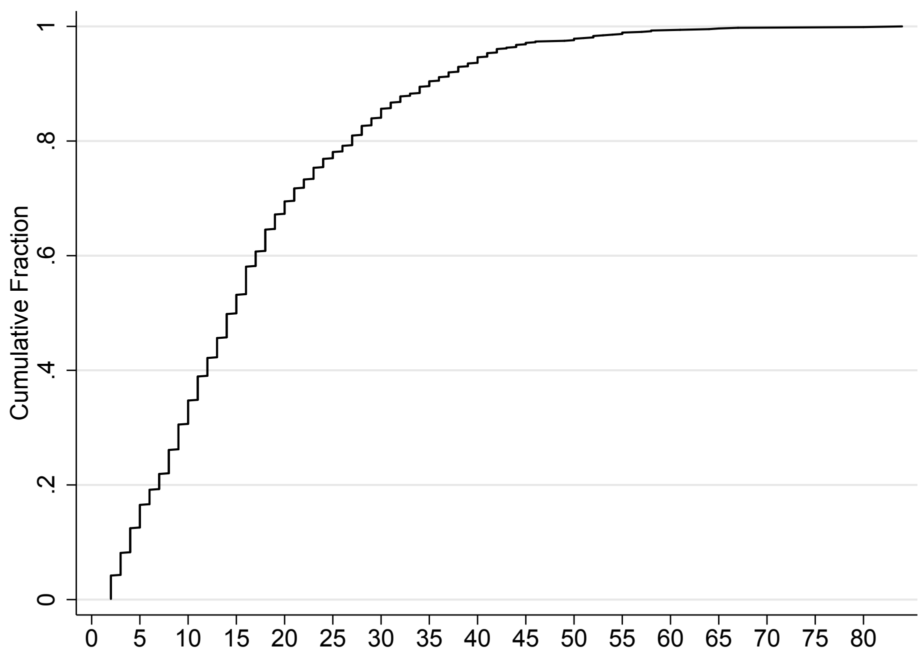
(b) Great Plains Whites

Notes: Figure B.1 displays the locally mean-smoothed relationship between the proportion living outside the South and age. See notes to figures 2.3a and 2.3b for definitions of home region.
Source: Authors' calculations using Ruggles et al. (2010) data

Figure B.2: Number of Towns per Birth Town Group, Cross Validation, Black Moves out of South



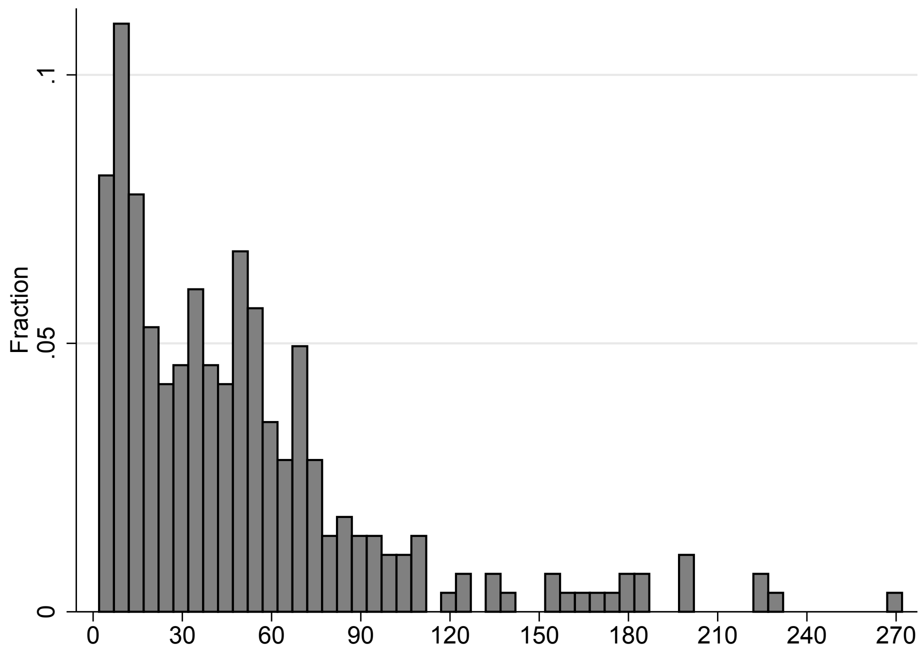
(a) Histogram



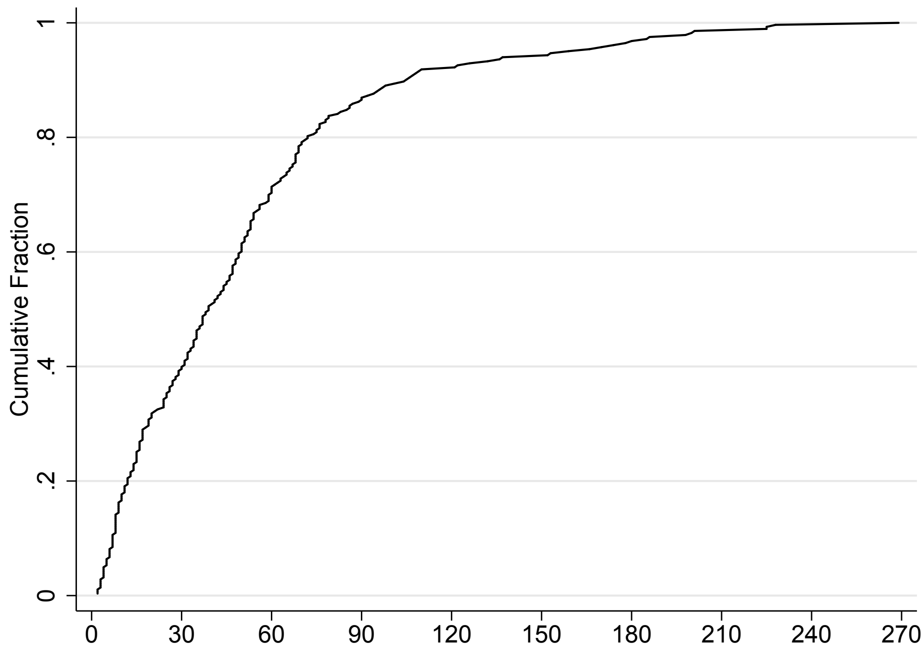
(b) Cumulative Distribution

Notes: Figure excludes groups with a single town, as these are not used in the analysis. Bin width in panel (a) is 1.
Source: Authors' calculations using Duke SSA/Medicare Data.

Figure B.3: Number of Towns per Birth Town Group, Cross Validation, White Moves out of Great Plains



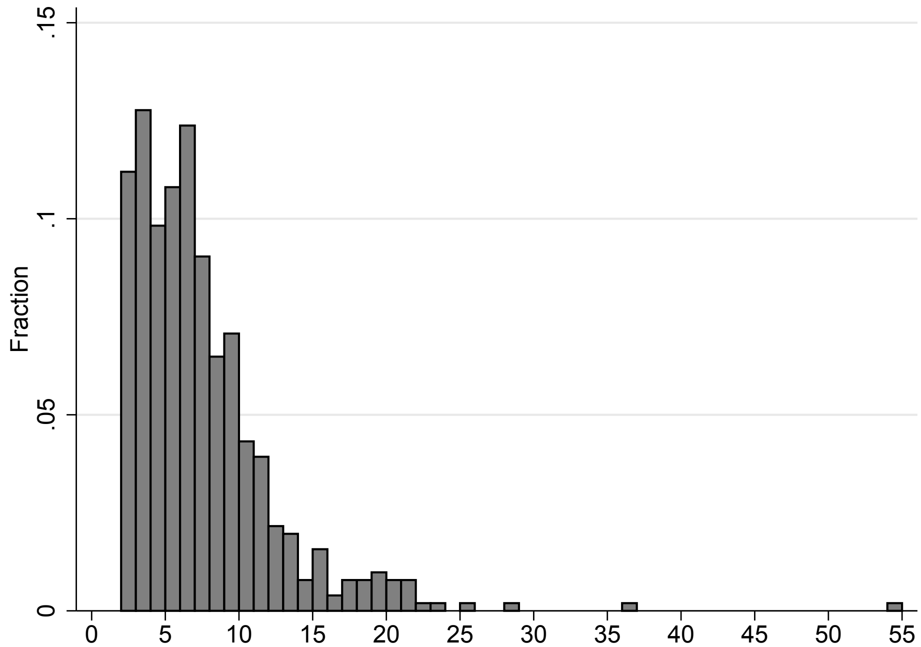
(a) Histogram



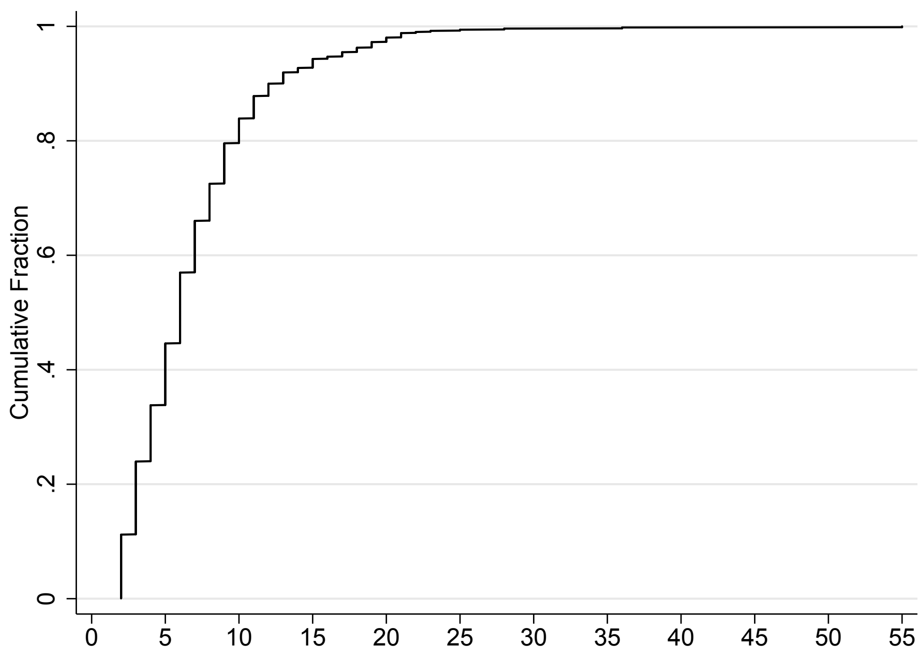
(b) Cumulative Distribution

Notes: Figure excludes groups with a single town, as these are not used in the analysis. Bin width in panel (a) is 5.
Source: Authors' calculations using Duke SSA/Medicare Data.

Figure B.4: Number of Towns per County, Black Moves out of South



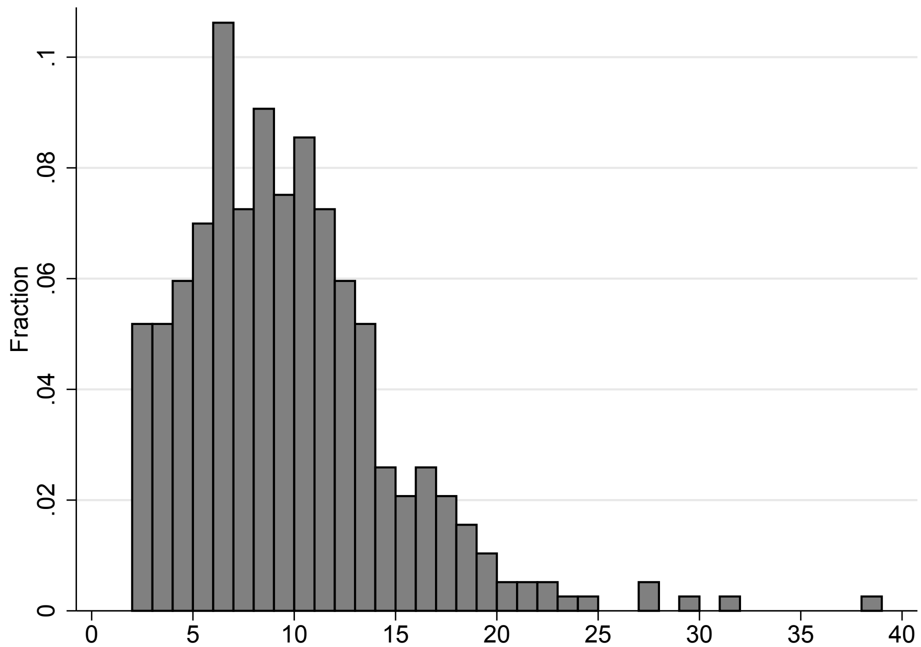
(a) Histogram



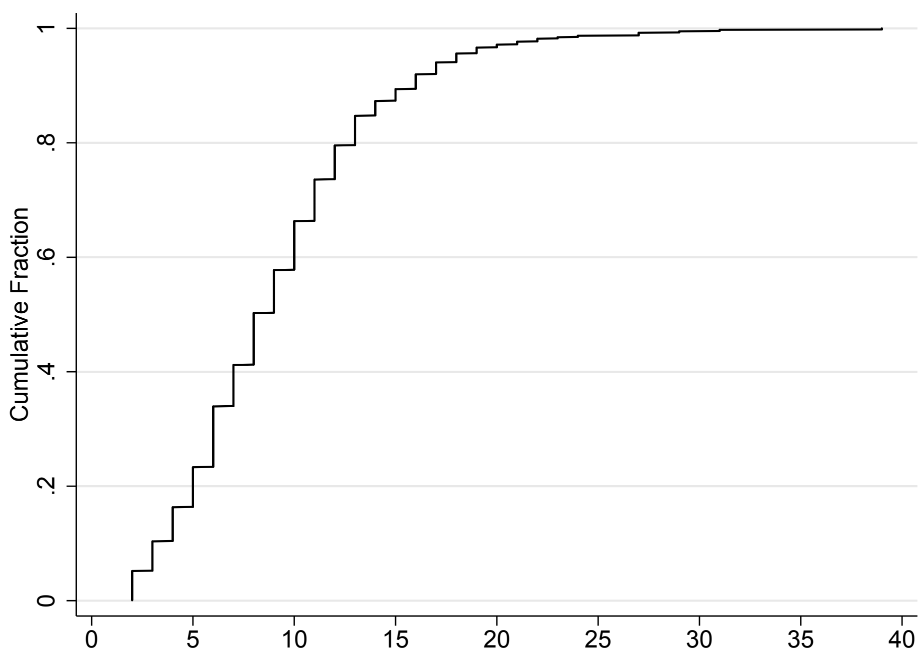
(b) Cumulative Distribution

Notes: Figure excludes groups with a single town, as these are not used in the analysis. Bin width in panel (a) is 1.
Source: Authors' calculations using Duke SSA/Medicare Data.

Figure B.5: Number of Towns per County, White Moves out of Great Plains



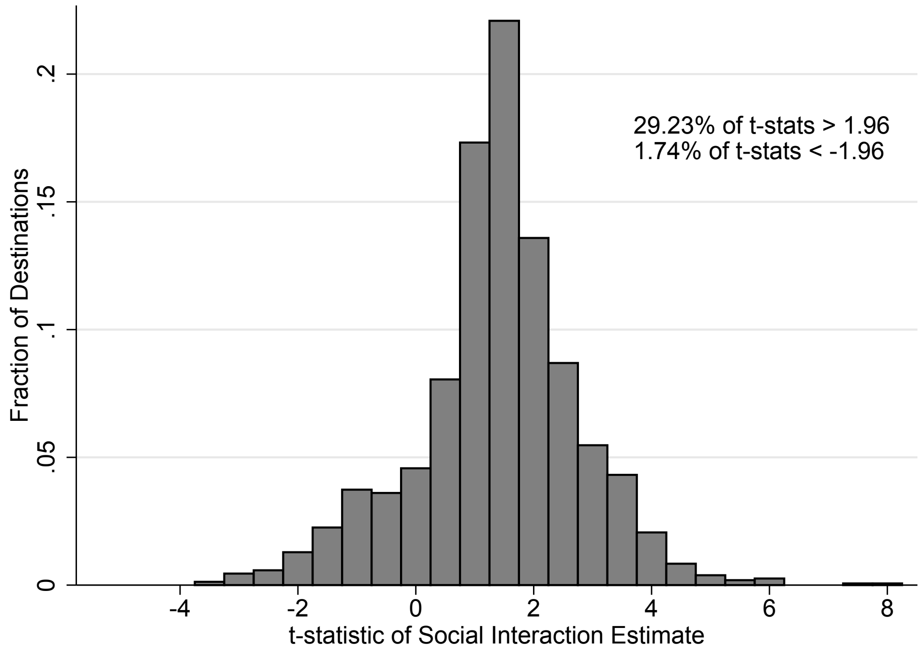
(a) Histogram



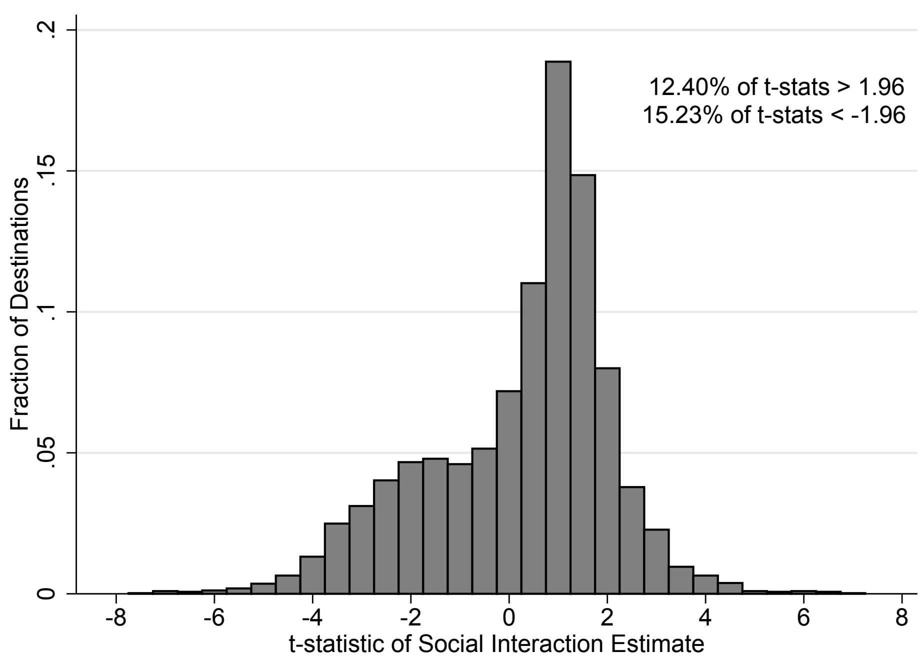
(b) Cumulative Distribution

Notes: Figure excludes groups with a single town, as these are not used in the analysis. Bin width in panel (a) is 1. Source: Authors' calculations using Duke SSA/Medicare Data.

Figure B.6: Distribution of Destination Level Social Interaction t-statistics



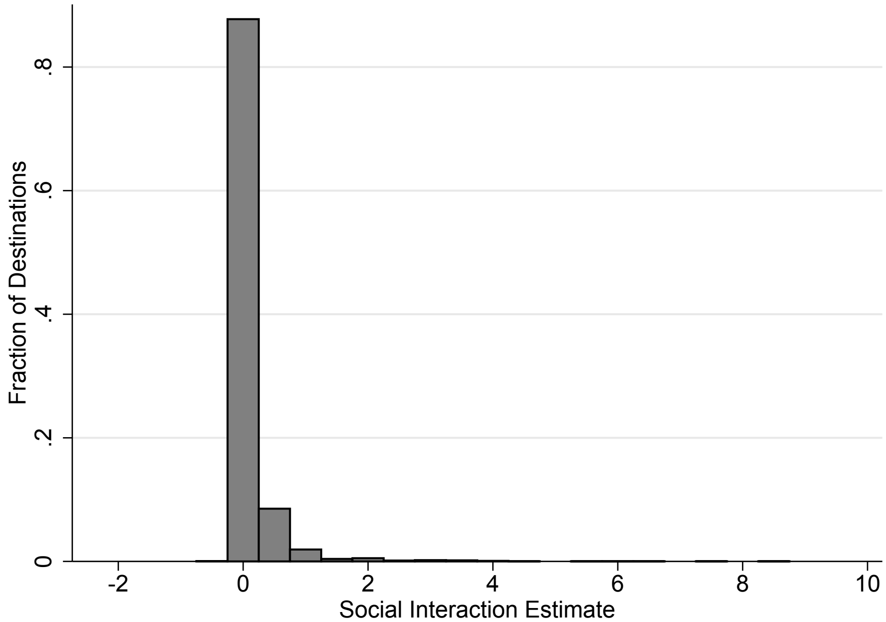
(a) Black Moves out of South



(b) White Moves out of Great Plains

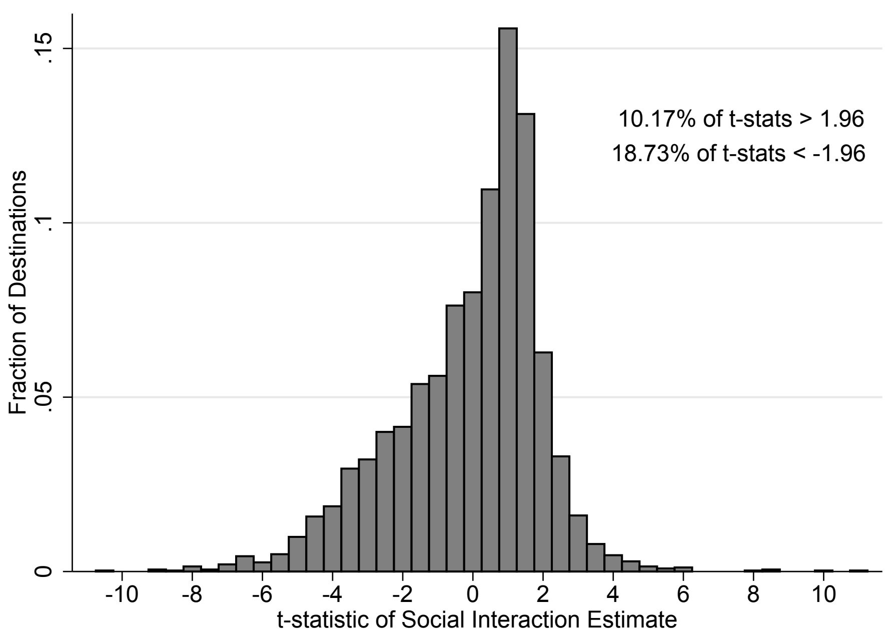
Notes: Bin width is 1/2. Birth town groups are defined by cross validation. Panel (a) omits the t-statistic of 13.7 from South Carolina to Hancock, WV.
Source: Authors' calculations using Duke SSA/Medicare data

Figure B.7: Distribution of Social Interaction Estimates, White Moves to North



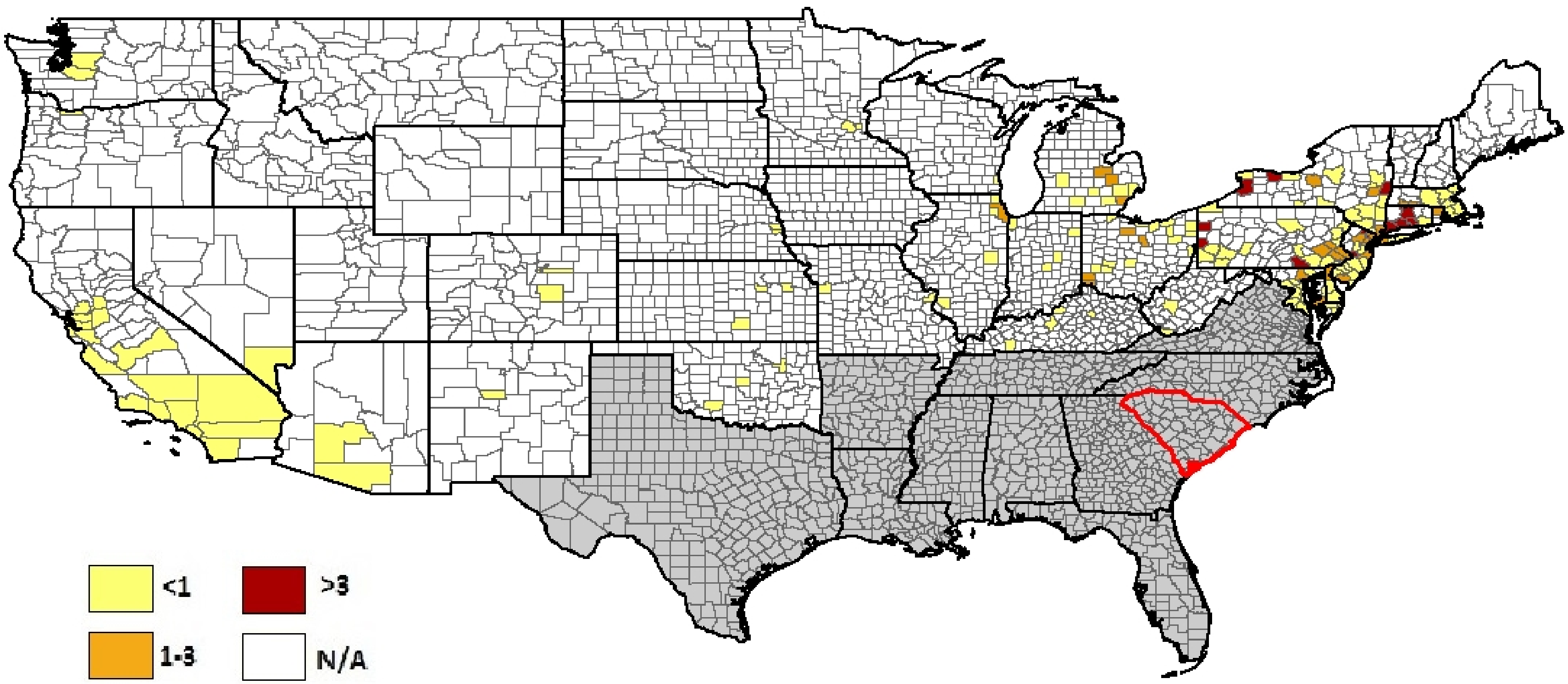
Note: Bin width is 1/2. Figure omits estimate of $\hat{\Delta}_k = 19.3$ from Alabama to St. Joseph County, IN.

Figure B.8: Distribution of Social Interaction t-statistics, White Moves to North



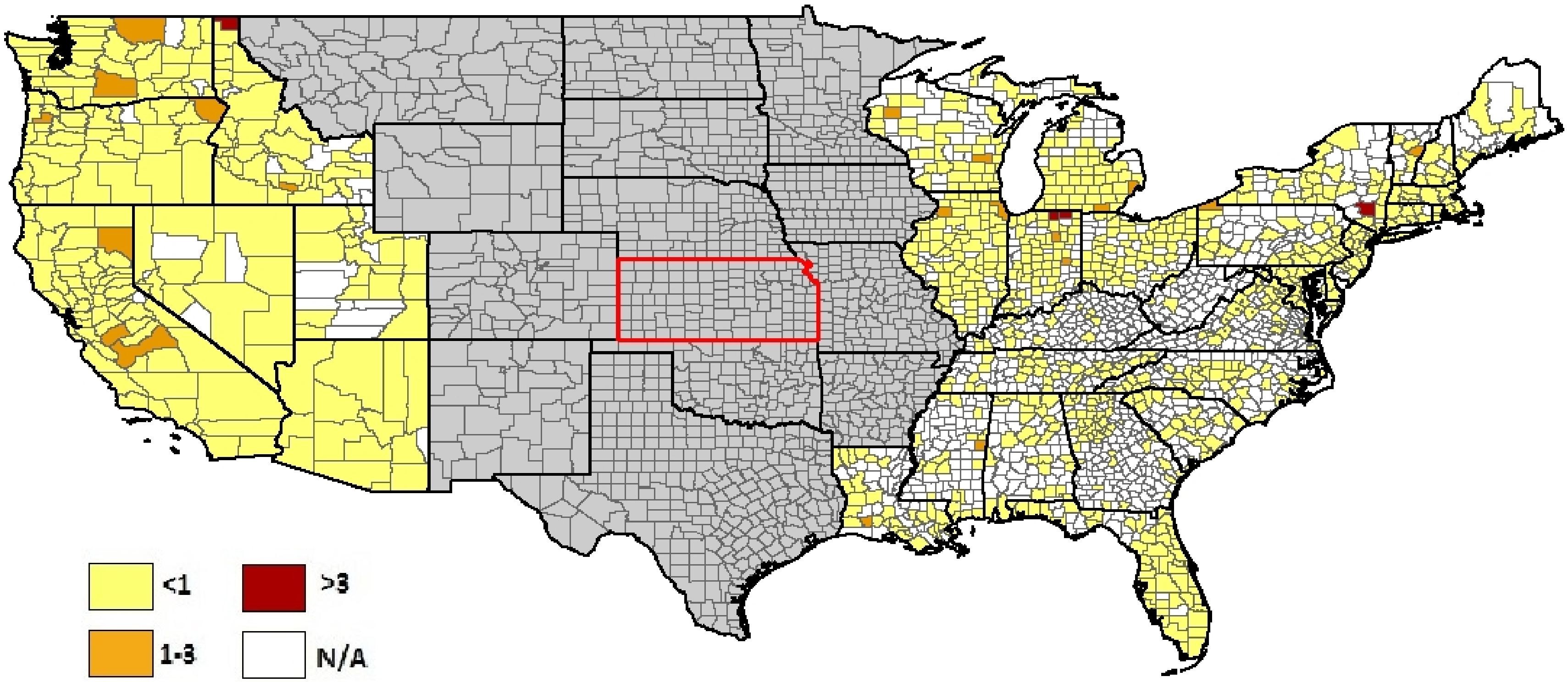
Note: Bin width is 1/2.

Figure B.9: Spatial Distribution of Destination-Level Social Interaction Estimates, South Carolina-born Blacks



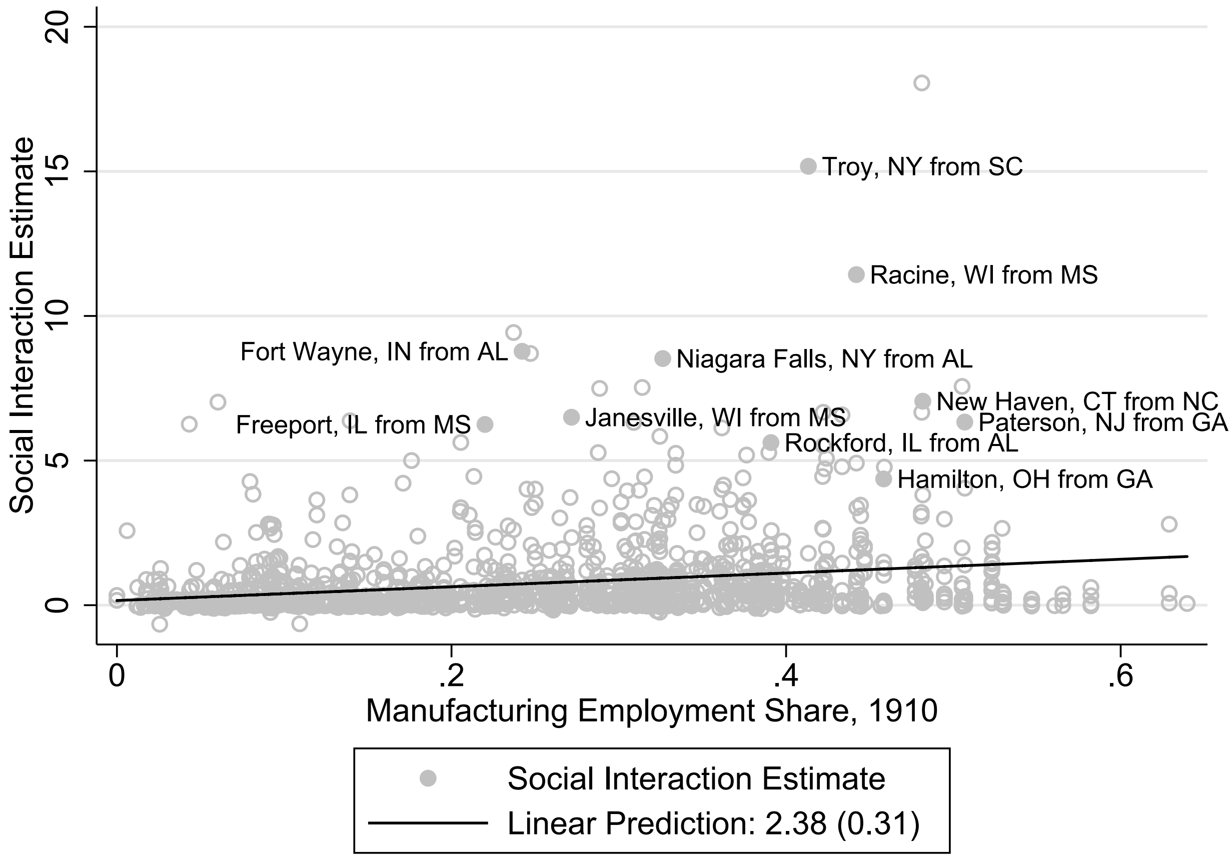
Notes: See note to Figure 2.5.

Figure B.10: Spatial Distribution of Destination-Level Social Interaction Estimates, Kansas-born Whites



Notes: See note to Figure 2.6.

Figure B.11: Relationship between Southern Black Destination Level Social Interaction Estimates and 1950 Manufacturing Employment Share



Note: Linear prediction comes from an OLS regression which includes a constant and 1910 manufacturing employment share. See table 2.7 for results when including a richer set of covariates. Listed are the cities in Table 2.2.

APPENDIX C

Appendix to Chapter 3

C.1 Theoretical Details

C.1.1 Proof of Proposition 1

To prove Proposition 1, we show that the assumptions of a stable equilibrium and non-negative peer effects (i.e., elements of J) imply that the peer effect multipliers m^s, m^n, and m^w are non-negative.

Let $\lambda_1, \lambda_2, \lambda_3$ be the eigenvalues of the 3×3 matrix J. The spectral radius of J is defined as $\rho(J) \equiv \max\{|\lambda_1|, |\lambda_2|, |\lambda_3|\}$. To ensure the equilibrium is stable, we assume that $\rho(J) < 1$. In each peer effect parametrization considered in Table 3.9, all eigenvalues are real and lie in $[0, 1)$, and this condition is satisfied.

The on-diagonal elements of J (J_{11}, J_{22}, J_{33}) are less than one in a stable equilibrium. This follows from the facts that the spectral radius is less than one if and only if $\lim_{k\to\infty} J^k = 0$ and $\lim_{k\to\infty} J^k = 0$ implies that the on-diagonal elements of J are less than one.

In a stable equilibrium, we also have that $\det(I - J) > 0$, where I is the 3×3 identity matrix. This follows from our assumption that $\rho(J) < 1$, the fact that $\det(J) = \lambda_1\lambda_2\lambda_3$, and the fact that $\det(J) = \lambda_1\lambda_2\lambda_3$ if and only if $\det(I - J) = (1 - \lambda_1)(1 - \lambda_2)(1 - \lambda_3)$.

It is straightforward to show that

$$\det(I - J) = (1 - J_{11})[(1 - J_{22})(1 - J_{33}) - J_{23}J_{32}] \tag{C.1}$$
$$- J_{12}[J_{23}J_{31} + J_{21}(1 - J_{33})] - J_{13}[J_{21}J_{32} + J_{31}(1 - J_{22})]$$
$$= (1 - J_{11})m^s - J_{12}m^n - J_{13}m^w, \tag{C.2}$$

where the second equality uses the peer effect multipliers defined in equations (3.7)-(3.9). Because the off-diagonal elements of J are non-negative (by assumption) and the on-diagonal elements of J are less than 1 (as implied by a stable equilibrium), we have that m^n and m^w are non-negative. As a result,

$$0 < \det(I - J) \leq (1 - J_{11})m^s. \tag{C.3}$$

Because $J_{11} < 1$, this implies that m^s is non-negative. QED.

C.1.2 Discussion of Proposition 2

As noted in the text, two jointly sufficient conditions for Proposition 2 are (a): $d\bar{C}^s/d\text{HHI}^s < d\bar{C}^w/d\text{HHI}^s$ and (b): $d\bar{C}^n/d\text{HHI}^s \leq d\bar{C}^w/d\text{HHI}^s$. Assuming that $\partial F^s/\partial\text{HHI}^s < 0$, conditions (a) and (b) are equivalent to $m^s > m^w$ and $m^n \geq m^w$. Rearranging equations (3.7) and (3.9) shows that condition (a) is satisfied if and only if

$$(1 - J_{22})(1 - J_{33}) > J_{32}(J_{21} + J_{23}) + J_{31}(1 - J_{22}). \tag{C.4}$$

The left hand side of inequality (C.4) is positive because $J_{22}, J_{33} \in [0, 1)$ in a stable equilibrium with non-negative peer effects. Hence, condition (a) will be true as long as cross-group peer effects, on the right hand side, are small enough.

Similarly, equations (3.8) and (3.9) imply that condition (b) is satisfied if and only if

$$J_{21}(1 - J_{33} - J_{32}) \geq J_{31}(1 - J_{22} - J_{23}). \tag{C.5}$$

If blacks with ties to the South have a larger peer effect on blacks without ties to the South than non-blacks, $J_{21} > J_{31} \geq 0$, then inequality (C.5) is satisfied if $(J_{22} - J_{33}) + (J_{23} - J_{32}) \geq 0$, which will hold insofar as own-group peer effects among blacks without ties to the South are at least as strong as own-group peer effects among non-blacks ($J_{22} \geq J_{33}$) and an increase in the non-black crime rate leads to a greater increase in the crime rate among blacks without ties to the South than vice versa ($J_{32} \geq J_{23}$), which is plausible because baseline crime rates are higher among blacks than non-blacks.

It is useful to consider the simple case where there are no cross-group peer effects between black and non-black youth, $J_{13} = J_{23} = J_{31} = J_{32} = 0$. In this case, the peer effect multipliers are

$$m^s = \frac{1 - J_{22}}{(1 - J_{11})(1 - J_{22}) - J_{12}J_{21}} \tag{C.6}$$

$$m^n = \frac{J_{21}}{(1 - J_{11})(1 - J_{22}) - J_{12}J_{21}} \tag{C.7}$$

$$m^w = 0 \tag{C.8}$$

In a stable equilibrium, $J_{22} \in [0, 1)$ and $(1 - J_{11})(1 - J_{22}) > J_{12}J_{21}$, ensuring that $m^s > m^w$ and condition (a) holds. Condition (b) additionally requires non-negative peer effects between blacks with and without ties to the South, $J_{21} \geq 0$.

C.2 Estimating a Model of Social Interactions in Location Decisions

Appendix C.2 describes a structural model of social interactions in location decisions. This model allows us to estimate the share of migrants that chose their destination because of social interactions. We include this variable in our regressions to examine whether the effect of social connectedness is driven by variation across cities in unobserved characteristics of migrants.

C.2.1 Model of Social Interactions in Location Decisions

Migrants from birth town j are indexed on a line by $i \in \{1, \ldots, N_j\}$, where N_j is the total number of migrants from town j. For migrant i, destination k belongs to one of three preference groups: high (H_i), medium (M_i), or low (L_i). The high preference group contains a single destination. In the absence of social interactions, the destination in H_i is most preferred, and destinations in M_i are preferred over those in L_i.[1] A migrant never moves to a destination in L_i. A migrant chooses a destination in M_i if and only if his neighbor, $i-1$, chooses the same destination. A migrant chooses a destination in H_i if his neighbor chooses the same destination or his neighbor selects a destination in L_i.[2]

Migrants from the same birth town can differ in their preferences over destinations. The probability that destination k is in the high preference group for a migrant from town j is $h_{j,k} \equiv \mathbb{P}[k \in H_i | i \in j]$, and the probability that destination k is in the medium preference group is $m_{j,k} \equiv \mathbb{P}[k \in M_i | i \in j]$.

Migrants with many destinations in their medium preference group will tend to be influenced by the decisions of other migrants. For our empirical work, distinguishing between types of migrants is important because migrants that are more influenced by social interactions might differ along several dimensions. For example, migrants with many destinations in their medium preference group might be negatively selected in terms of earnings ability or be more pro-social, as discussed in the text.

The probability that migrant i moves to destination k given that his neighbor moves there is

$$\rho_{j,k} \equiv \mathbb{P}[D_{i,j,k} = 1 | D_{i-1,j,k} = 1] = \mathbb{P}[k \in H_i] + \mathbb{P}[k \in M_i] \tag{C.9}$$

$$= h_{j,k} + m_{j,k}, \tag{C.10}$$

[1] The assumption that H_i is a non-empty singleton ensures that migrant i has a well-defined location decision in the absence of social interactions. We could allow H_i to contain many destinations and specify a decision rule among the elements of H_i. This extension would complicate the model without adding any new insights.

[2] This model shares a similar structure as Glaeser, Sacerdote and Scheinkman (1996) in that some agents imitate their neighbors. However, we differ from Glaeser, Sacerdote and Scheinkman (1996) in that we model the interdependence between various destinations (i.e., this is a multinomial choice problem) and allow for more than two types of agents.

where $D_{i,j,k}$ equals one if migrant i moves from j to k and zero otherwise.

The probability that destination k is in the medium preference group, conditional on not being in the high preference group, is $\nu_{j,k} \equiv \mathbb{P}[k \in M_i | k \notin H_i, i \in j]$. The conditional probability definition for $\nu_{j,k}$ implies that $m_{j,k} = \nu_{j,k}(1 - h_{j,k})$. We use $\nu_{j,k}$ to derive a simple sequential estimation approach.

In equilibrium, the probability that a randomly chosen migrant i moves from j to k is

$$\begin{aligned}
P_{j,k} \equiv \mathbb{P}[D_{i,j,k} = 1] = \mathbb{P}[D_{i-1,j,k} = 1, k \in H_i] + \mathbb{P}[D_{i-1,j,k} = 1, k \in M_i] \\
+ \sum_{k' \neq k} \mathbb{P}[D_{i-1,j,k'} = 1, k \in H_i, k' \in L_i] \qquad \text{(C.11)} \\
= P_{j,k} h_{j,k} + P_{j,k} \nu_{j,k} (1 - h_{j,k}) + \sum_{k' \neq k} P_{j,k'} h_{j,k} (1 - \nu_{j,k'}) \qquad \text{(C.12)} \\
= P_{j,k} \nu_{j,k} + \left(\sum_{k'=1}^{K} P_{j,k'} (1 - \nu_{j,k'}) \right) h_{j,k}. \qquad \text{(C.13)}
\end{aligned}$$

The first term on the right hand side of equation (C.11) is the probability that a migrant's neighbor moves to k, and k is in the migrant's high preference group; in this case, social interaction reinforces the migrant's desire to move to k. The second term is the probability that a migrant follows his neighbor to k because of social interactions. The third term is the probability that a migrant resists the pull of social interactions because town k is in the migrant's high preference group and the neighbor's chosen destination is in the migrant's low preference group.

The share of migrants from birth town j living in destination k that chose their destination because of social interactions equals $m_{j,k}$. As a result, the share of migrants in destination k that chose this destination because of social interactions is

$$m_k \equiv \sum_j N_{j,k} m_{j,k}, \qquad \text{(C.14)}$$

where $N_{j,k}$ is the number of migrants that moved from j to k. Our goal is to estimate m_k for each destination.

C.2.2 Estimation

To facilitate estimation, we connect this model to the social interactions (SI) index introduced by Stuart and Taylor (2017). The SI index is the expected increase in the number of people from birth town j that move to destination k when an arbitrarily chosen person i is observed to make the same move,

$$\Delta_{j,k} \equiv \mathbb{E}[N_{-i,j,k}|D_{i,j,k} = 1] - \mathbb{E}[N_{-i,j,k}|D_{i,j,k} = 0], \tag{C.15}$$

where $N_{-i,j,k}$ is the number of people who move from j to k, excluding person i. A positive value of $\Delta_{j,k}$ indicates positive social interactions in moving from j to k, while $\Delta_{j,k} = 0$ indicates the absence of social interactions. Stuart and Taylor (2017) show that the SI index can be expressed as

$$\Delta_{j,k} = \frac{C_{j,k}(N_j - 1)}{P_{j,k}(1 - P_{j,k})}, \tag{C.16}$$

where $C_{j,k}$ is the average covariance of location decisions between migrants from town j, $C_{j,k} \equiv \sum_{i \neq i' \in j} \mathbb{C}[D_{i,j,k}, D_{i',j,k}]/(N_j(N_j - 1))$. We follow the approach described in Stuart and Taylor (2017) to estimate $P_{j,k}$ and $\Delta_{j,k}$ using information on migrants' location decisions from the Duke SSA/Medicare data.[3]

The model implies that $C_{j,k}$ equals[4]

$$C_{j,k} = \frac{2P_{j,k}(1 - P_{j,k}) \sum_{s=1}^{N_j - 1} (N_j - s) \left(\frac{\rho_{j,k} - P_{j,k}}{1 - P_{j,k}} \right)^s}{N_j(N_j - 1)}. \tag{C.17}$$

Substituting equation (C.17) into equation (C.16) and simplifying yields[5]

$$\Delta_{j,k} = \frac{2(\rho_{j,k} - P_{j,k})}{1 - \rho_{j,k}}, \tag{C.18}$$

[3] We use cross validation to define birth town groups. See Stuart and Taylor (2017) for details.

[4] This follows from the fact that the covariance of location decisions for individuals i and $i + n$ is $\mathbb{C}[D_{i,j,k}, D_{i+n,j,k}] = P_{j,k}(1 - P_{j,k}) \left(\frac{\rho_{j,k} - P_{j,k}}{1 - P_{j,k}} \right)^n$.

[5] Equation (C.18) results from taking the limit as $N_j \to \infty$, and so relies on N_j being sufficiently large.

which can be rearranged to show that

$$\rho_{j,k} = \frac{2P_{j,k} + \Delta_{j,k}}{2 + \Delta_{j,k}}. \tag{C.19}$$

We use equation (C.19) to estimate $\rho_{j,k}$ with our estimates of $P_{j,k}$ and $\Delta_{j,k}$.

Equations (C.10) and (C.13), plus the fact that $m_{j,k} = \nu_{j,k}(1 - h_{j,k})$, imply that

$$\rho_{j,k} = \nu_{j,k} + \frac{P_{j,k}(1 - \nu_{j,k})^2}{\sum_{k'=1}^{K} P_{j,k'}(1 - \nu_{j,k'})}. \tag{C.20}$$

We use equation (C.20) to estimate $\nu_j \equiv (\nu_{j,1}, \ldots, \nu_{j,K})$ using our estimates of $(P_{j,1}, \ldots, P_{j,K}, \rho_{j,1}, \ldots, \rho_{j,K})$. We employ a computationally efficient algorithm that leverages the fact that equation (C.20) is a quadratic equation in $\nu_{j,k}$, conditional on $\sum_{k'=1}^{K} P_{j,k'}(1-\nu_{j,k'})$. We initially assume that $\sum_{k'=1}^{K} P_{j,k'}(1 - \nu_{j,k'}) = \sum_{k'=1}^{K} P_{j,k'} = 1$, then solve for $\nu_{j,k}$ using the quadratic formula, then construct an updated estimate of $\sum_{k'=1}^{K} P_{j,k'}(1 - \nu_{j,k'})$, and then solve again for $\nu_{j,k}$ using the quadratic formula. We require that each estimate of $\nu_{j,k}$ lies in $[0, 1]$. This iterated algorithm converges very rapidly in the vast majority of cases.[6]

We use equation (C.13) to estimate $h_{j,k}$ with our estimates of $\rho_{j,k}$ and $\nu_{j,k}$. Finally, we estimate $m_{j,k}$ using the fact that $m_{j,k} = \rho_{j,k} - h_{j,k}$. We use equation (C.14) to estimate our parameter of interest, m_k, using estimates of $m_{j,k}$ and observed migration flows, $N_{j,k}$.

C.2.3 Results

Appendix Figure C.2 displays a histogram of our estimates of the share of migrants that chose their destination because of social interactions, m_k, for cities in the North, Midwest, and West regions. The estimates range from 0 to 0.62. The unweighted average of m_k across cities is 0.26,

[6]For 10 birth towns, the algorithm does not converge because our estimates of $P_{j,k}$ and $\rho_{j,k}$ do not yield a real solution to the quadratic formula. We examined the sensitivity of our results to these cases by (1) dropping birth towns for which the algorithm did not converge, (2) estimating $\nu_{j,k}$ and $\sum_{k'=1}^{K} P_{j,k'}(1 - \nu_{j,k'})$ as the average of the values in the final four iterations, and (3) forcing $\hat{\nu}_{j,k}$ to equal zero for any (j, k) observation for which the quadratic formula solution does not exist. The motivation for (3) is that our estimates of $P_{j,k}$ and $\rho_{j,k}$ in these 10 cases were consistent with negative values of $\nu_{j,k}$, even though this was not a feasible solution. All three options yielded nearly identical estimates of our variable of interest, m_k. This is not surprising because these 10 birth towns account for a negligible share of the over 5,000 birth towns used to estimate m_k.

and the 1980 population weighted average is 0.39.

Appendix Table C.10 examines the relationship between log HHI, the log number of migrants, and m_k. The raw correlation between log HHI and m_k is negative, but when we control for the log number of migrants, log HHI and m_k are positively correlated, as expected. This relationship is similar when including state fixed effects.

Appendix Figure C.3 further describes the relationship between log HHI and m_k. Panel A plots the unconditional relationship between log HHI and m_k, while Panel B plots the relationship conditional on the log number of migrants.[7] When we control for m_k in equation (3.12), we identify the effect of social connectedness on crime using variation in the vertical dimension of Panel B.

Conditional on the number of migrants in a destination and the share of migrants that chose their destination because of social interactions, variation in social connectedness continues to arise from concentrated birth town to destination city population flows. To see this, consider two hypothetical cities that each have 20 migrants, one-fourth of whom chose their destination because of social interactions. In the low HHI city, the 20 migrants come from five birth towns. Each town sends four migrants, one of whom moves there because of social interactions. As a result, $\text{HHI}_{\text{Low}} = 0.2$. In the high HHI city, the 20 migrants also come from five birth towns. One town sends 12 migrants, three of whom move there because of social interactions. Two towns each send two migrants, one of whom moves there because of social interactions, and two towns each send two migrants, neither of whom is influenced by social interactions. As a result, $\text{HHI}_{\text{High}} = 0.4$.[8] This example is consistent with Figure 3.2 in that variation in social connectedness arises from the top sending town.

The structural model features local social interactions: each migrant directly influences no more than one migrant.[9] As a result, the model does not distinguish between the case where 12

[7] In particular, Panel B plots the residuals from regression log HHI and m_k on the log number of migrants.

[8] Alternatively, suppose that in the high HHI city, the 20 migrants come from three birth towns. One town sends 12 migrants, three of whom move there because of social interactions, and two towns each send four migrants, one of whom moves there because of social interactions. As a result, $\text{HHI}_{\text{High}} = 0.44$.

[9] However, a single migrant can indirectly influence several migrants.

migrants come from one town, with three migrants influenced by social interactions, and the case where 12 migrants come from three towns, with three migrants influenced by social interactions. Although this simple model does not capture all possible forms of social interactions, we believe that it likely captures the most relevant threats to our empirical strategy for this paper.

C.3 Details on Peer Effect Parametrization

Appendix C.3 provides additional details on the literature that guides our parametrization of peer effects in Section 3.6.

Case and Katz (1991) find that a one percent increase in the neighborhood crime rate leads to a 0.1 percent increase in a Boston youth's self-reported propensity of committing a crime during the last year (Table 10). This implies that a one percentage point increase in the neighborhood crime rate leads to a 0.1 percentage point increase in youth's crime rate, suggesting on-diagonal elements of J close to 0.1.

Glaeser, Sacerdote and Scheinkman (1996) estimate a local social interactions model in which there are two types of agents. Fixed agents are not affected by their peers, and compliers imitate their neighbor.[10] The probability that an agent is a complier thus maps to the on-diagonal elements of J. In Table IIA, the authors report estimates of $f(\pi) = (2 - \pi)/\pi$, where π is the probability that an agent is a fixed type. The probability that an agent is a complier is $1 - \pi = 1 - 2/(1 + f(\pi))$. Using FBI UCR data on murders across cities for 1970 and 1985, Glaeser, Sacerdote and Scheinkman (1996) report estimates of $f(\pi)$ between 2 and 4.5, implying on-diagonal elements of J between 1/3 and 2/3. For robbery and motor vehicle theft, the authors estimate $f(\pi)$ in the range of 37-155 and 141-382, suggesting diagonal elements of J very close to 1.

Ludwig and Kling (2007) find no evidence that neighborhood violent crime rates affect violent crime arrests among MTO participants age 15-25 (Table 4). These estimates suggest on-diagonal elements of J close to zero.

Damm and Dustmann (2014) estimate the effect of municipality crime rates on refugees' crim-

[10]Their model is similar to the one described in Appendix C.2.

inal convictions in Denmark. For males, they find that a one percentage point increase in the municipality crime rate leads to a 7-13 percent increase in the probability of conviction over a seven year period from ages 15-21 (Table 3, also see p. 1820). Given an average conviction rate of 46 percent, this translates into a 3-6 percentage point increase in the probability of conviction; we take the midpoint of 4.5. For females, the municipality crime rate has no effect on convictions. Consequently, these estimates imply that a one percentage point increase in the municipality crime rate leads to a $(0.5 \cdot 4.5)/7 \approx 0.32$ percentage point increase in refugees' annual conviction rate. This suggests on-diagonal elements of J close to 1/3. Damm and Dustmann (2014) find that, beyond the impact of the municipality crime rate, the crime rate of co-nationals has an additional impact while the crime rate of immigrants from other countries does not (Table 7). This suggests that cross-group peer effects might be small.

In sum, estimates from Case and Katz (1991) suggest on-diagonal values of J close to 0.1, estimates from Glaeser, Sacerdote and Scheinkman (1996) suggest on-diagonal elements of J close to 0.5 for murder, estimates from Ludwig and Kling (2007) suggest on-diagonal elements of J close to zero, and estimates from Damm and Dustmann (2014) suggest on-diagonal values of J close to 0.3 and off-diagonal elements near zero.

Table C.1: Summary Statistics: Crime and Social Connectedness, 1960-2009

	Mean	SD	First Quartile	Third Quartile	Fraction Zero
Offenses reported to police per 100,000 residents					
Murder	6.7	8.8	1.7	8.7	0.184
Rape	29	28	10	40	0.070
Robbery	215	252	68	270	0.004
Assault	1,134	1,099	287	1,622	0.005
Burglary	1,234	846	670	1,630	0.000
Larceny	3,228	1,785	2,023	4,198	0.000
Motor Vehicle Theft	582	513	260	742	0.000
Population	93,074	94,505	39,476	104,217	-
HHI, Southern Black Migrants	0.020	0.016	0.008	0.028	-
Log HHI, Southern Black Migrants	-4.220	0.781	-4.852	-3.563	-
Top Sending Town Share, Southern Black Migrants	0.061	0.041	0.036	0.074	-
Number, Southern Black Migrants	630	1,315	58	596	-

Notes: Each observation is a city-year. HHI and migrant counts are calculated among all individuals born in the former Confederacy states from 1916-1936. Data on rape is only available starting in 1964. Sample is restricted to cities with less than 500,000 residents in 1980.
Sources: FBI UCR, Duke SSA/Medicare dataset

Table C.2: Summary Statistics: Cities' Average Crime Rates

			Percentile				
	Mean	SD	5	25	50	75	95
Murder	6.7	6.8	1.3	2.7	4.5	8.0	19.2
Rape	29.1	18.3	6.5	16.0	26.3	36.9	65.8
Robbery	212.6	183.1	41.9	93.0	153.0	269.1	611.5
Assault	1,121.6	626.5	326.7	647.5	1,013.1	1,469.5	2,320.4
Burglary	1,233.1	474.0	541.8	891.9	1,185.3	1,510.2	2,095.9
Larceny	3,221.5	1,213.2	1,517.0	2,351.4	3,186.4	3,918.5	5,030.8
Motor Vehicle Theft	576.9	369.8	178.7	309.4	460.6	746.6	1,300.1

Notes: For each city, we construct an average crime rate across years 1960-2009. Table C.2 reports summary statistics of these average crime rates. Sample is restricted to cities with less than 500,000 residents in 1980.
Sources: FBI UCR

Table C.3: Summary Statistics: Cities With and Without 1911-1916 Homicide Rates

	1911-1916 Homicide Rates Observed	
	Yes	No
	(1)	(2)
HHI, Southern black migrants	0.007	0.021
	(0.006)	(0.016)
Number, Southern black migrants	7,999	540
	(16,068)	(2,079)
Population, 1980	549,344	80,839
	(1,099,422)	(170,680)
Percent black, 1980	0.237	0.103
	(0.152)	(0.148)
Percent female, 1980	0.530	0.519
	(0.008)	(0.019)
Percent 25+ with HS, 1980	0.489	0.560
	(0.080)	(0.098)
Percent 25+ with College, 1980	0.118	0.137
	(0.048)	(0.078)
Log area, square miles, 1980	3.886	2.729
	(0.986)	(0.888)
Log median family income, 1979	10.85	11.06
	(0.148)	(0.205)
Unemployment rate, 1980	0.0886	0.0708
	(0.033)	(0.030)
Labor force participation rate, 1980	0.458	0.483
	(0.041)	(0.052)
Manufacturing emp. share, 1980	0.213	0.233
	(0.072)	(0.094)
N (cities)	46	369

Notes: Table reports means and, in parentheses, standard deviations. Column 1 contains cities in the North, Midwest, and West regions that are in our main analysis sample and for which we observe homicide rates for at least one year from 1911-1916. These cities have at least 100,000 residents in 1920 and at least 5 deaths each year. Column 2 contains cities in the North, Midwest, and West regions that are in our main analysis sample but for which we do not observe homicide rates from 1911-1916. Unlike our main analysis sample, we do not restrict to cities with fewer than 500,000 residents in 1980.
Sources: Census (1922, p. 64-65) , Duke SSA/Medicare data, Census city data book

Table C.4: The Relationship between Social Connectedness and City Covariates, 1960-2009, Including African American-Specific Covariates

	Dependent variable: Log HHI, Southern black migrants			
Year covariates are measured:	1970	1980	1990	2000
	(1)	(2)	(3)	(4)
Log number, Southern black migrants	-0.806***	-0.779***	-0.744***	-0.750***
	(0.068)	(0.076)	(0.088)	(0.097)
Log population	-0.006	0.002	-0.022	0.025
	(0.073)	(0.078)	(0.089)	(0.089)
Percent black	-0.018	-0.000	-0.035	-0.075
	(0.059)	(0.077)	(0.073)	(0.066)
Percent female	-0.074	0.025	-0.008	0.018
	(0.060)	(0.079)	(0.089)	(0.076)
Percent age 5-17	-0.080	0.141	0.463*	0.448
	(0.226)	(0.262)	(0.267)	(0.332)
Percent age 18-64	-0.140	0.179	0.500*	0.577
	(0.235)	(0.277)	(0.280)	(0.365)
Percent age 65+	0.007	0.218	0.440**	0.444**
	(0.162)	(0.214)	(0.207)	(0.224)
Percent with high school degree	0.065	-0.131	0.017	-0.015
	(0.132)	(0.107)	(0.091)	(0.101)
Percent with college degree	0.027	0.017	-0.007	-0.016
	(0.073)	(0.054)	(0.082)	(0.086)
Log area, square miles	0.021	-0.028	-0.013	-0.028
	(0.062)	(0.070)	(0.077)	(0.083)
Log median family income	-0.075	-0.011	-0.202**	-0.067
	(0.096)	(0.089)	(0.099)	(0.082)
Unemployment rate	0.176**	-0.025	-0.070	0.029
	(0.083)	(0.087)	(0.092)	(0.058)
Labor force participation rate	0.073	0.007	0.085	-0.035
	(0.052)	(0.088)	(0.105)	(0.056)

Table C.4: The Relationship between Social Connectedness and City Covariates, 1960-2009, Including African American-Specific Covariates

	Dependent variable: Log HHI, Southern black migrants			
Year covariates are measured:	1970	1980	1990	2000
	(1)	(2)	(3)	(4)
Manufacturing employment share	0.203***	0.165***	0.163***	0.191***
	(0.065)	(0.059)	(0.053)	(0.047)
African American-Specific Covariates:				
Percent female	0.040	-0.085	0.012	0.077
	(0.046)	(0.062)	(0.074)	(0.072)
Percent age 5-17	0.122	0.098	0.160	-0.114
	(0.078)	(0.115)	(0.152)	(0.174)
Percent age 18-64	0.130	0.034	0.215	-0.025
	(0.088)	(0.131)	(0.180)	(0.212)
Percent age 65+	0.044	0.044	0.093	-0.017
	(0.055)	(0.070)	(0.087)	(0.103)
Percent with high school degree	-0.195***	-0.060	-0.112	-0.033
	(0.074)	(0.075)	(0.076)	(0.074)
Percent with college degree	0.160***	0.122*	0.125	0.059
	(0.053)	(0.064)	(0.079)	(0.079)
Unemployment rate	-0.083*	0.065	0.119**	0.101**
	(0.048)	(0.074)	(0.059)	(0.041)
State fixed effects	x	x	x	x
Adjusted R2	0.773	0.757	0.763	0.771
N (cities)	228	228	228	228
p-value: Wald test that parameters equal zero				
Demographic covariates	0.909	0.604	0.434	0.041
Economic covariates	0.023	0.990	0.220	0.521
African American-specific covariates	0.001	0.274	0.389	0.131

Notes: African American-specific covariates are not available for 1960. See note to Table 3.3.

Sources: Duke SSA/Medicare data, Census city data book, NHGIS

Table C.5: The Relationship between Social Connectedness and Measures of Social Capital

	Dependent variable: Log HHI, Southern black migrants							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: All Cities								
Associational density	0.0818	0.0601					0.135	0.109*
	(0.0571)	(0.0489)					(0.0908)	(0.0594)
Social capital index			0.0469	-0.00181			-0.0645	-0.0783
			(0.0558)	(0.0510)			(0.0920)	(0.0633)
Social capital composite index					0.0378	-0.00995		
					(0.0547)	(0.0477)		
Log number, Southern black migrants		-0.850***		-0.852***		-0.852***		-0.851***
		(0.0330)		(0.0324)		(0.0324)		(0.0331)
State fixed effects		x		x		x		x
R2	0.007	0.741	0.002	0.739	0.001	0.740	0.008	0.742
N (cities)	490	490	490	490	490	490	490	490
Counties	227	227	227	227	227	227	227	227
Panel B: Cities with Above Median Black Population Share in 1990								
Associational density	0.309***	0.118					0.514***	0.213**
	(0.0645)	(0.0746)					(0.103)	(0.103)
Social capital index			0.189***	0.0367			-0.264***	-0.149
			(0.0579)	(0.0767)			(0.0957)	(0.0979)
Social capital composite index					0.170***	0.0225		
					(0.0563)	(0.0719)		
Log number migrants		-0.629***		-0.653***		-0.655***		-0.621***
		(0.0600)		(0.0562)		(0.0559)		(0.0595)
State fixed effects		x		x		x		x
R2	0.129	0.598	0.043	0.591	0.034	0.590	0.155	0.603
N (cities)	229	229	229	229	229	229	229	229
Counties	152	152	152	152	152	152	152	152

Notes: All variables are normalized to have mean zero and standard deviation one in the sample used in Panel A. See Rupasingha and Goetz (2008) for definitions of associational density and social capital indices, which are measured at the county level using data from 1988 and 1990. The correlation between the social capital index and the social capital composite index is 0.99. Sample limited to cities with at least 25,000 residents in each decade and which received at least 25 Southern black migrants in the Duke dataset. Standard errors, clustered at the county level, are in parentheses. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

Sources: Duke SSA/Medicare data, Rupasingha and Goetz (2008)

Table C.6: The Effect of Social Connectedness on Crime, 1960-2009, Results for All Explanatory Variables

	Dependent variable: Number of offenses reported to police						
	Murder (1)	Rape (2)	Robbery (3)	Assault (4)	Burglary (5)	Larceny (6)	Motor Vehicle Theft (7)
Log HHI, Southern black migrants	-0.181***	-0.083**	-0.251***	-0.142***	-0.095***	-0.049	-0.163***
	(0.034)	(0.035)	(0.035)	(0.042)	(0.022)	(0.030)	(0.041)
Log number, Southern black migrants	0.150***	0.060**	0.146***	0.075**	0.051***	0.038	0.041
	(0.022)	(0.027)	(0.027)	(0.029)	(0.018)	(0.024)	(0.029)
Log population	0.944***	0.837***	1.118***	0.864***	0.947***	0.871***	1.273***
	(0.053)	(0.042)	(0.052)	(0.049)	(0.030)	(0.042)	(0.053)
Percent black, 1960	2.615***	3.717***	2.703***	3.520***	1.683***	0.588	1.585***
	(0.394)	(0.488)	(0.422)	(0.541)	(0.378)	(0.412)	(0.400)
Percent black, 1970	1.898***	2.512***	1.522***	0.890***	0.904***	0.066	1.204***
	(0.225)	(0.248)	(0.223)	(0.298)	(0.161)	(0.255)	(0.268)
Percent black, 1980	1.598***	1.556***	1.184***	0.592**	0.315**	-0.177	0.872***
	(0.167)	(0.162)	(0.192)	(0.265)	(0.140)	(0.243)	(0.235)
Percent black, 1990	1.544***	0.730***	0.737***	0.183	0.060	-0.085	0.616**
	(0.205)	(0.216)	(0.201)	(0.238)	(0.165)	(0.303)	(0.291)
Percent black, 2000	1.880***	0.117	0.418*	-0.132	0.127	-0.447*	0.890***
	(0.226)	(0.234)	(0.234)	(0.218)	(0.174)	(0.265)	(0.246)
Percent female, 1960	-0.235	2.965	-2.321	1.183	3.846	1.469	1.113
	(3.323)	(3.972)	(4.267)	(4.217)	(2.643)	(2.287)	(3.278)
Percent female, 1970	1.142	2.396	-0.379	-5.374*	-0.069	-0.241	1.260
	(1.880)	(1.971)	(2.195)	(2.880)	(1.258)	(1.451)	(2.595)
Percent female, 1980	-1.743	-1.131	-1.689	-4.141	1.588	-2.773	-0.973
	(2.047)	(2.317)	(2.549)	(3.038)	(1.574)	(2.143)	(3.114)
Percent female, 1990	-3.829	-2.197	0.538	-1.329	1.103	-1.298	4.573
	(2.706)	(2.904)	(3.728)	(2.574)	(2.226)	(2.251)	(4.266)
Percent female, 2000	4.335	1.984	-0.818	3.643*	-1.443	-0.649	-2.015
	(3.008)	(2.383)	(2.603)	(1.959)	(1.611)	(1.809)	(3.010)
Percent age 5-17, 1960	-1.476	-18.408***	0.751	-16.009**	1.816	-7.283**	6.275
	(5.192)	(5.431)	(6.667)	(6.454)	(3.536)	(3.305)	(4.411)
Percent age 18-64, 1960	-1.143	-11.610**	4.168	-8.046	1.531	-6.607***	5.548*
	(4.056)	(4.685)	(5.295)	(4.982)	(2.750)	(2.448)	(3.371)

Table C.6: The Effect of Social Connectedness on Crime, 1960-2009, Results for All Explanatory Variables

	Dependent variable: Number of offenses reported to police						
	Murder	Rape	Robbery	Assault	Burglary	Larceny	Motor Vehicle Theft
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Percent age 65+, 1960	-2.843	-13.297***	0.545	-14.016***	-0.145	-5.873***	0.182
	(3.270)	(3.851)	(4.903)	(4.282)	(2.601)	(2.092)	(3.248)
Percent age 5-17, 1970	-6.603**	-9.194***	-7.336**	-7.073	-3.975**	-3.004	-0.515
	(2.937)	(2.969)	(3.033)	(4.493)	(1.811)	(2.151)	(3.307)
Percent age 18-64, 1970	-3.771	-4.638*	-3.465	-7.827*	-4.797***	-3.551*	1.888
	(2.705)	(2.514)	(2.751)	(4.153)	(1.588)	(1.913)	(2.775)
Percent age 65+, 1970	-4.117*	-7.088***	-4.046*	-5.228	-3.043**	-2.272	-1.347
	(2.255)	(2.167)	(2.413)	(3.303)	(1.414)	(1.600)	(2.709)
Percent age 5-17, 1980	-8.082***	-10.612***	-3.334	-12.578***	-6.098**	1.356	11.437***
	(2.917)	(2.932)	(4.021)	(4.662)	(2.709)	(4.058)	(4.338)
Percent age 18-64, 1980	-9.361***	-8.200***	-3.751	-11.294***	-5.998***	-0.036	8.985***
	(2.162)	(2.090)	(2.854)	(3.314)	(1.903)	(2.330)	(3.225)
Percent age 65+, 1980	-4.834**	-7.669***	-0.178	-7.982**	-3.899**	2.976	10.184***
	(2.421)	(2.327)	(3.241)	(3.659)	(1.902)	(3.708)	(3.435)
Percent age 5-17, 1990	-17.701***	-9.090**	-7.317*	-8.706*	-4.683*	1.342	6.294
	(4.289)	(4.108)	(4.114)	(4.456)	(2.632)	(3.324)	(5.232)
Percent age 18-64, 1990	-14.688***	-7.455***	-4.407*	-7.640**	-6.078***	0.464	6.159*
	(2.996)	(2.697)	(2.587)	(3.152)	(1.865)	(2.536)	(3.250)
Percent age 65+, 1990	-10.878***	-6.553**	-3.425	-6.599**	-3.676*	2.157	5.563
	(3.419)	(3.059)	(3.106)	(3.335)	(1.923)	(2.183)	(3.845)
Percent age 5-17, 2000	-4.741	-9.525*	-2.977	-0.087	6.760**	2.669	8.752*
	(5.067)	(5.145)	(4.226)	(4.047)	(3.400)	(4.091)	(5.190)
Percent age 18-64, 2000	-5.702	-6.522	-2.049	-1.315	5.537**	2.441	9.519**
	(3.819)	(4.205)	(3.511)	(3.163)	(2.731)	(3.220)	(4.100)
Percent age 65+, 2000	-4.116	-6.737*	-1.575	0.202	6.110**	2.847	7.808**
	(3.921)	(3.900)	(3.226)	(3.061)	(2.590)	(3.131)	(3.827)
Percent with high school degree, 1960	-1.444**	-0.341	0.134	-0.178	0.041	-0.487	-1.186
	(0.631)	(0.651)	(0.878)	(0.806)	(0.572)	(0.663)	(0.722)
Percent with high school degree, 1970	-2.494***	-1.387***	-1.844***	-3.207***	-0.832**	-0.171	-2.596***
	(0.566)	(0.499)	(0.570)	(0.616)	(0.325)	(0.385)	(0.667)

Table C.6: The Effect of Social Connectedness on Crime, 1960-2009, Results for All Explanatory Variables

	Dependent variable: Number of offenses reported to police						
	Murder (1)	Rape (2)	Robbery (3)	Assault (4)	Burglary (5)	Larceny (6)	Motor Vehicle Theft (7)
Percent with high school degree, 1980	-2.298***	-0.405	-1.495**	-1.244*	-1.413***	-1.353**	-1.145*
	(0.528)	(0.472)	(0.653)	(0.673)	(0.335)	(0.553)	(0.625)
Percent with high school degree, 1990	-1.893***	1.513***	-1.325**	1.097**	0.841**	0.542	-1.125*
	(0.470)	(0.466)	(0.531)	(0.500)	(0.366)	(0.420)	(0.645)
Percent with high school degree, 2000	-1.397***	2.796***	-0.705	1.561***	1.419***	1.033***	-0.636
	(0.507)	(0.530)	(0.553)	(0.456)	(0.370)	(0.390)	(0.609)
Percent with college degree, 1960	-0.425	1.146	-1.973*	-0.447	0.849	2.421***	0.168
	(1.061)	(1.349)	(1.178)	(1.405)	(0.793)	(0.698)	(1.187)
Percent with college degree, 1970	-0.308	1.252**	-0.221	2.245***	1.548***	1.605***	0.316
	(0.765)	(0.609)	(0.764)	(0.686)	(0.370)	(0.387)	(0.802)
Percent with college degree, 1980	0.420	0.032	0.187	0.244	0.875***	1.434***	-1.306*
	(0.482)	(0.484)	(0.596)	(0.700)	(0.326)	(0.402)	(0.725)
Percent with college degree, 1990	-0.324	-0.574	-0.046	-0.661*	0.725***	0.911***	-1.505***
	(0.414)	(0.376)	(0.373)	(0.361)	(0.281)	(0.285)	(0.548)
Percent with college degree, 2000	0.035	-1.091**	-0.081	-0.320	-0.065	0.615*	-2.208***
	(0.456)	(0.501)	(0.448)	(0.422)	(0.339)	(0.320)	(0.621)
Log area, square miles, 1960	-0.004	0.282***	-0.108	0.080	0.048	0.060	-0.169***
	(0.059)	(0.058)	(0.084)	(0.070)	(0.043)	(0.045)	(0.058)
Log area, square miles, 1970	0.042	0.270***	-0.136**	0.127***	0.063***	0.090**	-0.218***
	(0.052)	(0.040)	(0.053)	(0.047)	(0.024)	(0.039)	(0.048)
Log area, square miles, 1980	0.098*	0.272***	-0.105*	0.127***	0.086***	0.133***	-0.186***
	(0.051)	(0.038)	(0.056)	(0.044)	(0.026)	(0.034)	(0.052)
Log area, square miles, 1990	0.092*	0.183***	-0.126**	0.113***	0.081***	0.125***	-0.054
	(0.047)	(0.040)	(0.053)	(0.043)	(0.029)	(0.037)	(0.052)
Log area, square miles, 2000	0.067	0.121***	-0.188***	0.098**	0.061**	0.106***	-0.127***
	(0.049)	(0.040)	(0.048)	(0.042)	(0.029)	(0.040)	(0.044)
Log median family income, 1960	-1.335**	-0.763	-0.736	-0.848	-1.117***	-0.585*	-0.477
	(0.527)	(0.665)	(0.686)	(0.688)	(0.371)	(0.325)	(0.537)
Log median family income, 1970	-0.434	-0.983***	-0.264	-0.049	-0.757***	-0.848***	0.635
	(0.298)	(0.294)	(0.369)	(0.373)	(0.196)	(0.198)	(0.388)

Table C.6: The Effect of Social Connectedness on Crime, 1960-2009, Results for All Explanatory Variables

	Dependent variable: Number of offenses reported to police						
	Murder (1)	Rape (2)	Robbery (3)	Assault (4)	Burglary (5)	Larceny (6)	Motor Vehicle Theft (7)
Log median family income, 1980	-0.783***	-1.525***	-0.953***	-0.468	-0.377*	-0.866***	0.028
	(0.216)	(0.241)	(0.342)	(0.361)	(0.217)	(0.235)	(0.355)
Log median family income, 1990	-0.512**	-1.912***	-1.030***	-1.319***	-1.215***	-1.517***	-0.280
	(0.260)	(0.240)	(0.315)	(0.260)	(0.165)	(0.186)	(0.382)
Log median family income, 2000	-1.281***	-2.149***	-1.227***	-1.722***	-1.310***	-1.216***	-0.616***
	(0.189)	(0.197)	(0.152)	(0.174)	(0.153)	(0.160)	(0.229)
Unemployment rate, 1960	-0.628	2.086	6.734**	3.018	2.871	2.433	1.905
	(2.272)	(3.165)	(3.431)	(3.369)	(2.125)	(1.977)	(2.538)
Unemployment rate, 1970	-0.603	-1.855	0.905	1.376	-0.356	-0.128	0.883
	(1.686)	(1.635)	(2.171)	(2.114)	(1.257)	(1.270)	(2.256)
Unemployment rate, 1980	1.473	2.048*	-0.629	2.811*	2.180**	2.787***	1.122
	(1.306)	(1.132)	(1.503)	(1.534)	(0.977)	(0.895)	(1.801)
Unemployment rate, 1990	6.720***	0.768	2.448*	0.672	3.206**	-1.041	2.081
	(2.130)	(1.735)	(1.451)	(1.651)	(1.247)	(1.658)	(2.566)
Unemployment rate, 2000	-1.312	-1.369	-2.271*	0.627	2.313**	2.087*	-0.583
	(1.587)	(1.384)	(1.285)	(0.932)	(1.072)	(1.104)	(1.107)
Labor force participation rate, 1960	4.029*	3.201	5.054**	4.236**	3.114**	2.727***	2.575
	(2.162)	(2.349)	(2.143)	(2.016)	(1.291)	(0.989)	(1.599)
Labor force participation rate, 1970	1.072	1.114	2.498**	3.674***	1.987***	1.827**	0.845
	(1.102)	(0.911)	(1.260)	(1.398)	(0.623)	(0.760)	(1.283)
Labor force participation rate, 1980	2.912***	3.393***	3.105**	3.142**	2.077***	4.067***	1.398
	(1.012)	(0.945)	(1.351)	(1.506)	(0.668)	(1.138)	(1.370)
Labor force participation rate, 1990	2.653***	2.965***	3.234**	2.009**	2.280***	3.077***	1.682
	(0.985)	(1.017)	(1.401)	(0.966)	(0.765)	(0.833)	(1.559)
Labor force participation rate, 2000	0.545	1.144***	1.137***	1.371***	0.223	1.266***	0.238
	(0.429)	(0.372)	(0.388)	(0.300)	(0.302)	(0.325)	(0.482)
Manufacturing employment share, 1960	0.022	0.724	0.969**	1.489***	0.314	-0.069	0.000
	(0.344)	(0.451)	(0.479)	(0.515)	(0.308)	(0.280)	(0.405)
Manufacturing employment share, 1970	0.058	0.476	0.170	0.141	0.062	-0.161	-0.398
	(0.292)	(0.293)	(0.340)	(0.430)	(0.192)	(0.230)	(0.321)

Table C.6: The Effect of Social Connectedness on Crime, 1960-2009, Results for All Explanatory Variables

	Dependent variable: Number of offenses reported to police						
	Murder (1)	Rape (2)	Robbery (3)	Assault (4)	Burglary (5)	Larceny (6)	Motor Vehicle Theft (7)
Manufacturing employment share, 1980	0.619**	0.063	0.239	-0.049	-0.300	-0.832**	0.106
	(0.298)	(0.278)	(0.377)	(0.463)	(0.259)	(0.419)	(0.452)
Manufacturing employment share, 1990	0.294	0.209	0.371	0.197	0.370	-0.255	0.002
	(0.350)	(0.360)	(0.381)	(0.425)	(0.320)	(0.423)	(0.465)
Manufacturing employment share, 2000	0.322	0.988**	0.068	0.688	0.641**	0.415	-0.118
	(0.388)	(0.447)	(0.372)	(0.429)	(0.323)	(0.314)	(0.515)
State fixed effects	x	x	x	x	x	x	x
Pseudo R2	0.773	0.838	0.931	0.913	0.938	0.926	0.906
N (city-years)	18,854	17,690	18,854	18,854	18,854	18,854	18,854
Cities	471	471	471	471	471	471	471

Notes: See note to Table 3.4.
Sources: FBI UCR, Duke SSA/Medicare data, Census city data book

Table C.7: The Effect of Social Connectedness on Crime, 2000-2009, by Predicted Crimes

	Dependent variable: Number of offenses reported to police						
	Murder (1)	Rape (2)	Robbery (3)	Assault (4)	Burglary (5)	Larceny (6)	Motor Vehicle Theft (7)
All Cities	-0.091	0.078	-0.074	-0.129**	0.002	-0.029	-0.011
	(0.071)	(0.078)	(0.058)	(0.059)	(0.044)	(0.044)	(0.064)
Below Median Predicted Crimes	-0.064	0.171	0.190	-0.041	0.021	0.065	0.285**
	(0.162)	(0.144)	(0.138)	(0.120)	(0.075)	(0.073)	(0.114)
Above Median Predicted Crimes	-0.073	0.044	-0.047	-0.167***	-0.034	-0.042	-0.015
	(0.075)	(0.090)	(0.062)	(0.064)	(0.045)	(0.049)	(0.071)

Notes: Table displays estimates of equation (3.12). Sample restricted to cities with less than 500,000 residents in 1980. Regressions include the same covariates used in Table 3.4. To generate the predicted number of crimes for each city, we estimate equation (3.12) using data from 1995-1999, replacing state-year fixed effects with state-specific linear time trends. We then predict the number of crimes with these coefficients and covariates from 2000-2009, using the average value of log HHI and log number of migrants for all cities when generating the prediction. We estimate regressions using data from 2000-2009. Standard errors, clustered at the city level, are in parentheses. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$
Sources: FBI UCR, Duke SSA/Medicare data, Census city data book

Table C.8: Negative Selection of Southern Black Migrants into Network Destinations

Sample:	Men and Women			Men			Women		
Dependent variable:	Years of Schooling	Log Income	Log Income	Years of Schooling	Log Income	Log Income	Years of Schooling	Log Income	Log Income
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A: Selection into state of residence									
Share of migrants from birth	-1.594***	-0.107***	-0.041	-1.768***	-0.058**	0.019	-1.516***	-0.025	0.090*
state in state of residence	(0.154)	(0.031)	(0.030)	(0.176)	(0.022)	(0.019)	(0.152)	(0.051)	(0.052)
Years of schooling			0.041***			0.044***			0.076***
			(0.002)			(0.001)			(0.005)
N	97,132	77,760	77,760	45,187	42,960	42,960	51,945	34,800	34,800
R2	0.080	0.084	0.099	0.082	0.120	0.147	0.082	0.110	0.150
Panel B: Selection into metropolitan area of residence									
Share of migrants from birth	-1.990***	-0.182***	-0.108**	-2.057***	-0.118***	-0.036	-1.995***	-0.154***	-0.002
state in metro of residence	(0.117)	(0.044)	(0.044)	(0.108)	(0.035)	(0.036)	(0.154)	(0.057)	(0.059)
Years of schooling			0.036***			0.039***			0.070***
			(0.002)			(0.001)			(0.006)
N	66,359	52,958	52,958	30,533	29,201	29,201	35,826	23,757	23,757
R2	0.084	0.070	0.081	0.086	0.102	0.125	0.088	0.096	0.131
Quartic in age	x	x	x	x	x	x	x	x	x
Year of birth fixed effects	x	x	x	x	x	x	x	x	x
Birth state fixed effects	x	x	x	x	x	x	x	x	x
State/metro of residence fixed effects	x	x	x	x	x	x	x	x	x
Year fixed effects	x	x	x	x	x	x	x	x	x

Notes: Sample limited to African Americans born in the South from 1916-1936 who are living in the North, Midwest, or West regions. Standard errors, clustered at the state of residence level, are in parentheses. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

Sources: 1960 and 1970 Census IPUMS

Table C.9: The Effect of Social Connectedness on Crime, 1960-2009, Additional Robustness Checks

	Dependent variable: Number of offenses reported to police						
	Murder	Rape	Robbery	Assault	Burglary	Larceny	Motor Vehicle Theft
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: Including cities with at least 500,000 residents in 1980							
Log HHI, Southern	-0.168***	-0.159***	-0.187***	-0.194***	-0.139***	-0.120***	-0.235***
black migrants	(0.036)	(0.037)	(0.039)	(0.043)	(0.026)	(0.029)	(0.039)
Pseudo R2	0.935	0.921	0.983	0.947	0.974	0.971	0.968
N (city-years)	19,543	18,324	19,543	19,543	19,543	19,543	19,543
Cities	485	485	485	485	485	485	485
Panel B: Negative binomial model							
Log HHI, Southern	-0.120***	-0.052	-0.129***	-0.079**	-0.039	-0.037	-0.115***
black migrants	(0.032)	(0.032)	(0.039)	(0.036)	(0.027)	(0.029)	(0.043)
Pseudo R2	0.283	0.217	0.187	0.143	0.148	0.123	0.144
N (city-years)	18,854	17,690	18,854	18,854	18,854	18,854	18,854
Cities	471	471	471	471	471	471	471
Panel C: Drop observations if dependent variable is below 1/6 or above 6 times city mean							
Log HHI, Southern	-0.128***	-0.076**	-0.247***	-0.133***	-0.091***	-0.045	-0.158***
black migrants	(0.031)	(0.036)	(0.034)	(0.042)	(0.022)	(0.029)	(0.041)
Pseudo R2	0.766	0.846	0.935	0.902	0.943	0.933	0.910
N (city-years)	15,192	15,695	17,823	15,250	18,712	18,715	18,613
Cities	470	471	471	471	471	471	471
Panel D: Drop observations if dependent variable is below 1/6 or above 6 times city median							
Log HHI, Southern	-0.156***	-0.080**	-0.246***	-0.133***	-0.090***	-0.044	-0.158***
black migrants	(0.032)	(0.036)	(0.034)	(0.042)	(0.022)	(0.029)	(0.041)
Pseudo R2	0.776	0.848	0.935	0.901	0.943	0.933	0.909
N (city-years)	15,711	15,799	17,844	15,246	18,705	18,693	18,652
Cities	471	470	471	471	471	471	471
Panel E: Measure HHI using birth county to destination city population flows							
Log HHI, Southern	-0.154***	-0.053	-0.214***	-0.120***	-0.066***	-0.042	-0.137***
black migrants	(0.033)	(0.032)	(0.038)	(0.039)	(0.023)	(0.032)	(0.041)
Pseudo R2	0.772	0.837	0.930	0.913	0.937	0.926	0.906
N (city-years)	18,854	17,690	18,854	18,854	18,854	18,854	18,854
Cities	471	471	471	471	471	471	471

Notes: In Panel B, we estimate a negative binomial model instead of equation (3.12). For Panels C and D, we construct mean and median number of crimes for each city from 1960-2009. Regressions include the same covariates used in Table 3.4. Standard errors, clustered at the city level, are in parentheses. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

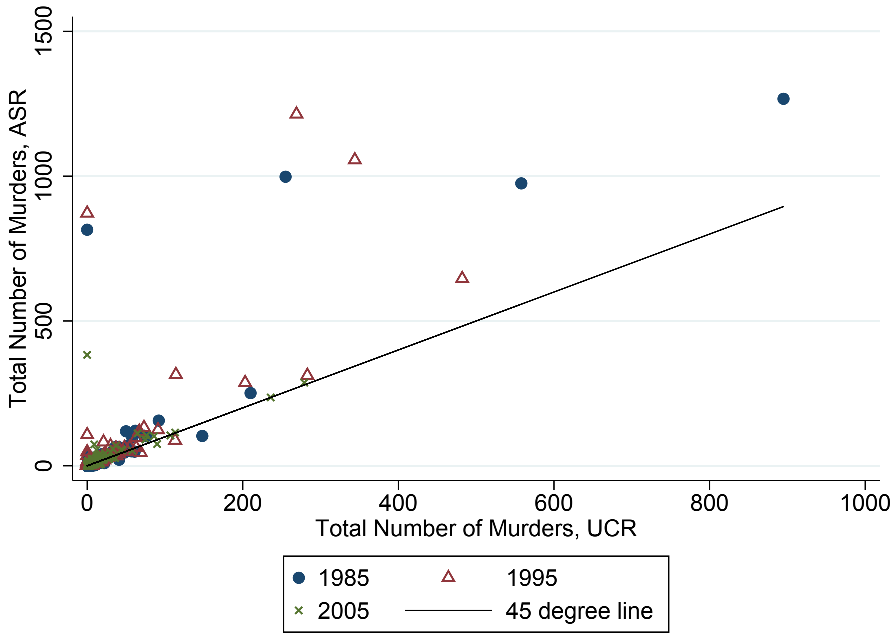
Sources: FBI UCR, Duke SSA/Medicare data, Census city data book

Table C.10: The Relationship between Social Connectedness, the Number of Migrants, and the Share of Migrants that Chose their Destination Because of Social Interactions

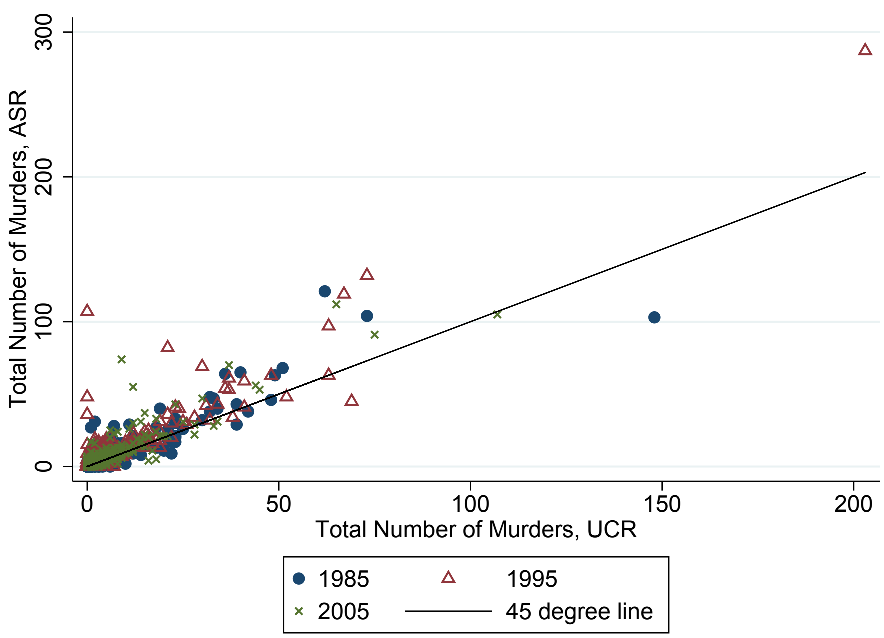
	Dependent variable: Log HHI, Southern black migrants			
	(1)	(2)	(3)	(4)
Log number, Southern black migrants	-0.457***		-0.666***	-0.669***
	(0.014)		(0.021)	(0.023)
Share of migrants that chose destination because of social interactions		-2.423***	2.896***	2.993***
		(0.282)	(0.229)	(0.259)
State fixed effects				x
R2	0.723	0.184	0.834	0.848
N (cities)	471	471	471	471

Notes: Sample restricted to cities with less than 500,000 residents in 1980. We estimate the share of migrants that chose their destination because of social interactions using a structural model, as described in the text.
Sources: Duke SSA/Medicare data,

Figure C.1: The Relationship between Murder Counts from Different FBI Data Sets



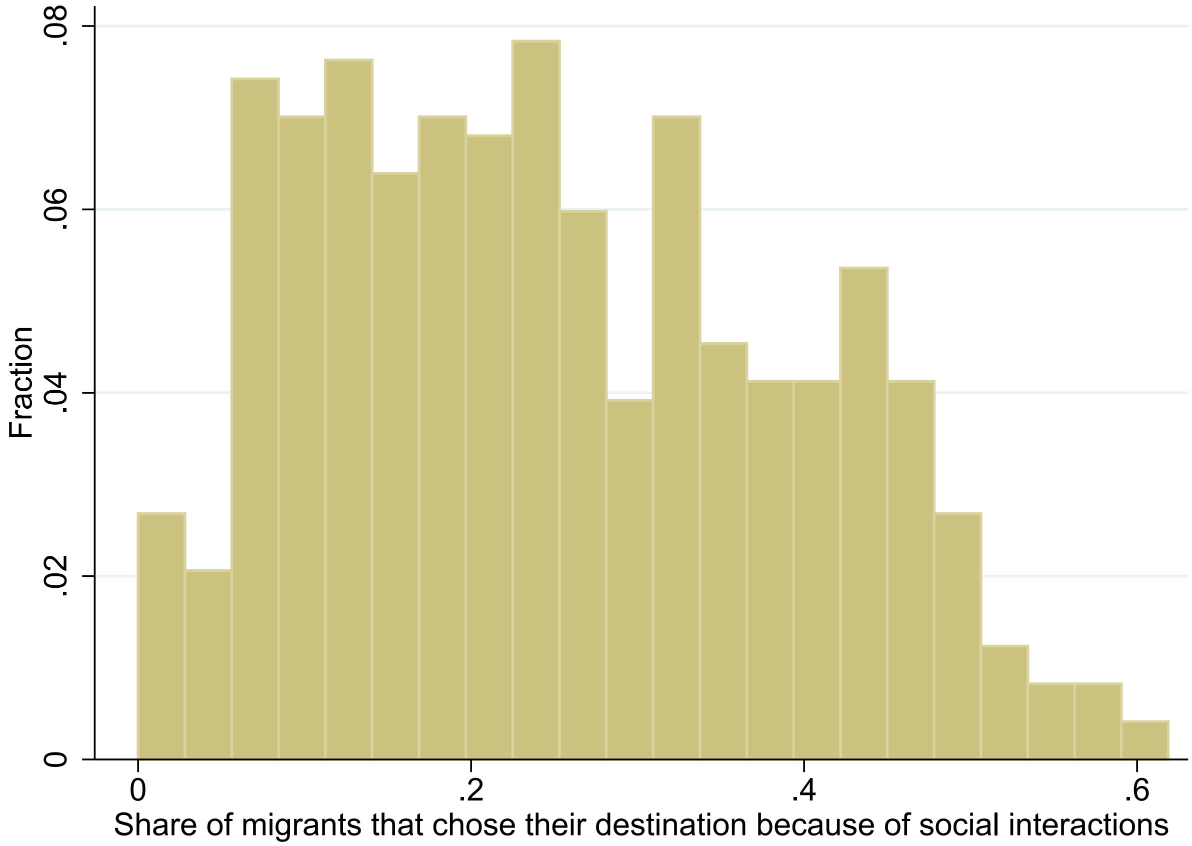
(a) All cities



(b) Cities with less than 500,000 residents in 1980

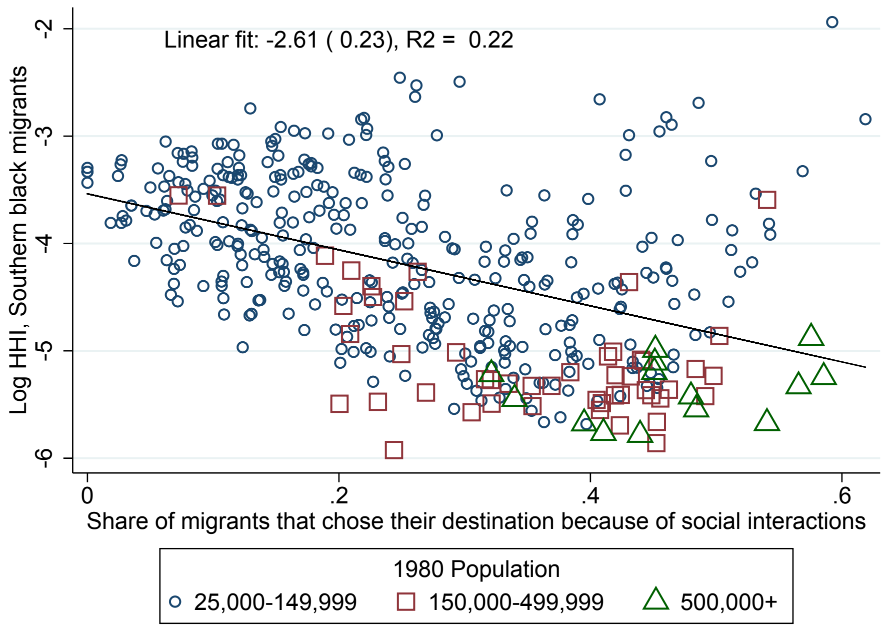
Notes: The UCR data contain the total number of murders per police agency. To construct a similar measure from the ASR data, we calculate the sum of murders committed by adult whites, adult blacks, adult other races, juvenile whites, juvenile blacks, and juvenile other races.
Source: FBI UCR

Figure C.2: Share of Migrants that Chose their Destination Because of Social Interactions

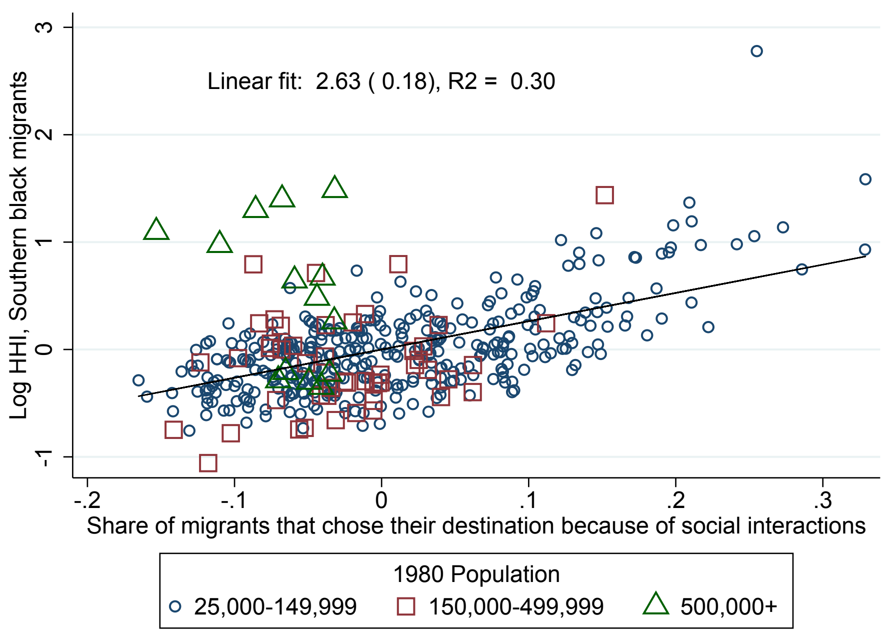


Notes: We estimate the share of migrants that chose their destination because of social interactions using a structural model, as described in the text.
Source: Duke SSA/Medicare data

Figure C.3: The Relationship between Social Connectedness and the Share of Migrants that Chose their Destination Because of Social Interactions



(a) Raw



(b) Conditional on Log Number, Southern Black Migrants

Notes: We estimate the share of migrants that chose their destination because of social interactions using a structural model, as described in the text. Panel B plots the residuals from regressing log HHI and the share of migrants that chose their destination because of social interactions on the log number of migrants.
Source: Duke SSA/Medicare data

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